THE

EDINBURGH MEDICAL AND SURGICAL JOURNAL:

EXHIBITING

A CONCISE VIEW

OF THE

LATEST AND MOST IMPORTANT DISCOVERIES

IN

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VOLUME TWENTY-FIRST.

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The following Publications have been received—

General Anatomy applied to Physiology and the Practice of Medicine. By X. Bichat. Translated from the last French edition by Constant Coffyn; revised and corrected by George Calvert, Esq. Member of the Royal College of Surgeons. Part I. including the two first volumes. pp. 743. 8vo. London, 1824.

The Animal Kingdom Arranged in Conformity with its Organization. By the Baron Cuvier. With Additional Descriptions of the Species hitherto named; of many not before noticed, and other original Matter. By Edward Griffith, F.L.S. and others. pp. 203. With 17 Plates. 8vo. London, 1824.

Pathological Observations on the Rotated or Contorted Spine, commonly called Curvature, deduced from Practice; in which are shown, the Causes that produce it; the Reason of its being mistaken for an Incurvation of the Spinal Column; and the Means best adapted to its Prevention and Cure; agreeably to the Principles laid down, and the Author's experience. By Andrew Dods, M.D., late of Edinburgh, and Surgeon of the Royal Navy. pp. 239. 8vo. London, 1824.

An Essay on an Improved Method of Cutting for Urinary Calculi, or the posterior operation of Lithotomy. By W. W. Sleigh, M.R.C.S., and Lecturer on Anatomy. pp. 104. 8vo. London, 1824.

Essay on the Effects of Iodine on the Human Constitution; with Practical Observations on its Use in the Cure of Bronchocele, Scrophula, and the Tuberculous Diseases of the Chest and Abdomen. By W. Gairdner, M.D. pp. 64. 8vo. London, 1824.

The Medico-Chirurgical Review, and Journal of Medical Science (Quarterly), conducted by associated Physicians and Surgeons, and superintended by James Johnson, M.D., Member of the Royal College of Physicians of London, &c. &c.

Elements of Medical Jurisprudence. By Theodoric Romeyn Beck, M.D., Professor of the Institutes of Medicine, and Lecturer on Medical Jurisprudence, in the College of Physicians and Surgeons in the Western District of the State of New-York. 2 vols. 8vo. Albany, 1823.

Observations on Prison Discipline, exemplified by the Tread-Mill and Dietary adopted in the Nottinghamshire House of Correction at Southwell. By Benjamin Hutchinson, Surgeon to the Establishment. 8vo. Newark, 1823.

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A Statement of the Transactions on occasion of the extraordinary Sickness which has lately occurred at Millbank. By A. Copland Hutchison, Surgeon, R. N., Senior Surgeon to the Westminster General Dispensary. 8vo. London, 1823.

Observations on the prevailing Practice of supplying Medical Assistance to the Poor, commonly called The Farming of Parishes; with Suggestions for the Establishment of Parochial Medical Chests, or Infirmaries, in Agricultural Districts. By Henry Lilley Smith, Surgeon, Southam. 8vo. London, 1819.

An Appeal to the Public and to the Legislature, on the necessity of affording Dead Bodies to the Schools of Anatomy, by Legislative Enactment. By William Mackenzie, Andersonian Professor of Anatomy and Surgery, Member of the Royal College of Surgeons of London, &c. &c. 8vo. Glasgow, 1824.

Die Krankheiten des menschlichen Auges; ein Handbuch für angehende Aerzte: Nach den besten in—und ausländischen Werken, mit besonderer Berücksichtigung der Beer'schen Enfahrungen bearbeitet und durch eigene Beobachtungen vermehrt: Herausgegeben von Dr Carl. Heinrich Weller. Zweite verbesserte und vermehrte Auflage. 8vo. Berlin, 1822.

The American Medical Recorder of Original Papers, and Intelligence in Medicine and Surgery, conducted by an Association of Physicians in Philadelphia, Baltimore, and Norfolk. Quarterly Series. No. 24. Vol. IV. No. 4. 8vo. Philadelphia, October 1823.

The Monthly Gazette of Health; or Popular Medical, Dietetic, Philosophical and Anti-empirical Journal; edited by Dr Richard Reece of London. Vol. VIII. for 1823. 8vo. London, 1823.

The Lancet. Vol. I. and part of Vol. II.

Just published, The New Medico-Chirurgical Pharmacopœia, being a Selection of Modern Formulæ, from the private and hospital Practice of the most Eminent Members of the Profession, in Europe and America. For the Use of Surgeons and Surgeon-Apothecaries. Third Edition, with considerable Additions. By a Graduate of a Scotch University, Member of the College of Surgeons of London, and of the Society of Practical Medicine of Paris, &c. &c. London, 1824. pp. 180. 5s.

Also, A Translation of the New London Pharmacopœia, by a Scotch Physician, specifying the Doses of each Article, and the Diseases for the Cure of which they are administered, either alone, or in Combination with other Articles. Price 3s. 6d.

ERRATA.

p. 95, line 10, for *guttatim*, read *paullatim*. p. 97, line 14, for They, *read* The tendons. p. 399, line 10, for 36, *read* 54.

In a few copies only,—

p. 395, line 29, for enumeration, *read* examination.
400, — 7, for *conici*, read *conii*.

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1. JANUARY, 1824.

PART I.

ORIGINAL COMMUNICATIONS.

I.

CONTRIBUTIONS TO THE PATHOLOGY OF THE STOMACH, THE PANCREAS, AND THE SPLEEN.

By John Abercrombie, M. D.

Part I.

INFLAMMATORY AFFECTIONS AND ULCERATION OF THE STOMACH.

Acute inflammation of the stomach is not a common disease in this country; and, when it does occur, the symptoms are so severe and so well defined, as immediately to indicate the nature of the affection. But the stomach is liable to inflammatory action in a chronic form, which often advances so slowly and insidiously, that the dangerous nature of it may be overlooked, until it arrives at that period of its progress in which it assumes the characters of organic and hopeless disease. In the early stages of this affection, the prominent symptoms are often such as merely indicate derangement of the functions of the stomach, and are consequently very apt to be included under the general term Dyspepsia. The patient complains of acidity, eructations, flatulence, and oppression of the stomach after eating. There is usually pain, but it varies very much both in degree and in its duration. In many cases it is only complained of after eating, continues in considerable severity while the process of digestion is going on, and subsides after the process is completed. The appetite in these cases is frequently unimpaired; but the patient is afraid of taking food on account of the pain which is induced by it. In other cases, there is permanent uneasiness, but it is aggravated by taking food; frequently there is tenderness of the epigastric region on pressure; and sometimes there is pain in the back, at the part corresponding with the seat of the stomach. Vomiting is apt to occur; but in the early stages it is only occasional, and is ascribed to incidental causes. Afterwards it becomes more frequent, but still without that degree of regularity which appears to indicate serious disease. By great attention to diet, it may occur but seldom, and the disease may go on for several months without exciting alarm. The vomiting then becomes gradually more frequent, and the uneasiness in the stomach more permanent, until the patient either sinks by gradual wasting, or is cut off more rapidly by hæmatemesis, in some cases before the disease has been of long standing. In some of these cases there is little pain, the prominent symptom being the attacks of vomiting, which became gradually more and more frequent. In other cases, vomiting does not occur till a very advanced period, the leading symptoms being the pain, excited or increased after eating, with severe dyspeptic symptoms, and gradual wasting.

The disease which is going on during this course of symptoms, consists of chronic inflammation, which in many cases appears to commence in a circumscribed portion of the mucous membrane of the stomach. The progress of it appears to be extremely slow; and it is probable that it may continue for some time, and then subside, and appear again after a considerable interval, until it at last gradually produces more permanent effects, in thickening of the coats of the stomach, adhesion to the neighbouring parts, and ulceration.

The following case affords an example of this insidious and dangerous affection; and it is remarkable from the long intervals of health which repeatedly occurred in the progress of it.

Case I.—A woman, aged 45, had been for several years liable to attacks of pain in the stomach, which gradually became more severe and of longer continuance, until at length they continued for several weeks at a time, and were little affected by any remedies. I saw her in several of these attacks during the last eighteen months of her life, and, while affected by them, she complained of pain in the epigastric region, chiefly referred to a particular spot of small extent. It was very much increased by eating, so that her only relief was when the stomach was empty, and was accompanied by frequent vomiting, which, however, did not occur at any regular periods. These attacks usually continued for several weeks at a time, and then left her for weeks or months in very good health. Three weeks before her death, after having been for several months free from any uneasiness, she was exposed to cold by getting her feet wet, and almost immediately complained of uneasiness at the stomach. At first it was slight, with loss of appetite, but, after several days, it became more severe, with some vomiting. There was no fever; and nothing could be discovered by examination. The pain was chiefly referred to a particular spot in the epigastric region, rather to the left side; and no symptom occurred different from those in her former attacks, until after eight or ten days, she was suddenly seized with copious hæmatemesis. This occurred repeatedly; and she died rather suddenly about a week from the first occurrence of it, and about three weeks from the commencement of the attack. *Dissection.*—The stomach had contracted an adhesion of small extent to the left lobe of the liver, and another of greater extent, and very firm, to the pancreas. At both these places the coats of the stomach were diseased, but in the greatest degree at the adhesion to the pancreas. Here they were much thickened and indurated, for a space about three inches long and two inches broad; and the internal surface of this portion was entirely ulcerated, as if a portion of its thickness had been dissected out. Besides this uniform ulceration, there were several points which penetrated deeper, and some of these contained coagulated blood, giving reason to suppose that they had been the source of the hemorrhage. The remainder of the stomach was in a natural state, and the other viscera were healthy.

In this remarkable case, there is every reason to believe that the paroxysms of pain in the stomach had been connected with inflammatory action, confined to a certain circumscribed portion of it, and frequently subsiding after some continuance, leaving the part in a comparatively healthy state; but that under these successive attacks, the part had become gradually thickened, until the last attack terminated by ulceration, and this by the fatal hemorrhage.

On account of the periodical nature of the pain, and the long intervals of perfect health, I consider this case as one of very great interest. In the more common forms of the disease, the symptoms are more constant, become gradually more severe, and are, in the early periods, often so obscure, that, for a considerable time, the case may be considered as merely dyspeptic. A woman mentioned by Chardel, had dyspeptic complaints, with pain in the stomach and back, and occasional vomiting. Solids only were vomited; and, by great attention to diet, she suffered little inconvenience for several months. The vomiting then became more frequent; at length it occurred daily—and several times a day. She then wasted, and died, gradually exhausted, about eight months from the commencement of the disease, and between two and three months from the time when the vomiting began to occur every day. The stomach was found adhering to the liver, the spleen and the pancreas,—along the great arch it was considerably thickened, and internally ulcerated, and beset with granulations. There was ulceration also about the cardia. A man mentioned by the same writer had, for five months, vomiting after meals; and death, by gradual wasting, without any other symptom. Nothing could be felt in the region of the stomach by the most careful examination. On dissection, the stomach was found adhering intimately to the concave surface of the liver; and an ulcer at this place had perforated the stomach, and penetrated a considerable way into the substance of the liver. There was also ulceration in the neighbourhood of the cardia. A man, mentioned by Pinel, had great acidity of the stomach, and other dyspeptic symptoms, with occasionally attacks of acute pain, and afterwards vomiting and gradual wasting. A tumour was felt in the epigastric region. The pain became more acute, the smallest quantity of food producing great uneasiness; and he died exhausted, after six months. The omentum was found hard, red and fleshy, and gathered up into a mass under the great arch of the stomach. The mucous membrane of the stomach was much destroyed; and there was an ulcer three inches in length near the pylorus. Another course of symptoms occurs in a case related by Frank. A man, aged 50, was seized, after a violent exertion, with copious vomiting of blood, followed by discharge of blood by stool. These symptoms continued several weeks, and then ceased. He then had dyspeptic symptoms, debility and emaciation. His appetite was good, but he had great uneasiness after eating; and some tenderness was felt in the right hypochondrium, with difficulty of lying on the right side. He became gradually more and more emaciated; and at last had some vomiting and dropsical symptoms, and died after seven or eight months. On dissection, the liver was found pale, hard, and much diminished in size. The small curvature of the stomach adhered to it; and at the place of adhesion, there was an ulcer five inches long and two broad, which perforated the stomach, and penetrated into the substance of the liver.

An important variety in the history of the disease occurs in those cases which are distinguished merely by pain of the stomach and dyspeptic symptoms, without vomiting, or any symptom distinctly indicating serious disease. A man mentioned by Chardel had dyspepsia, and acute pain after eating, which subsided when the process of digestion was completed. By taking very mild food in small quantities, he felt less uneasiness; but after some time, without any change in the symptoms, he lost strength so much, that he was confined to bed. His appetite continued, but he was afraid to satisfy it. He had very little nausea, and did not vomit above two or three times during his illness, which continued many months. On dissection, there was found thickening of the coats of the stomach, at the upper part, without ulceration; and enlargement of the lymphatic glands in the neighbourhood of the stomach. A man mentioned by the same writer had pain in the right hypochondrium, loss of appetite, with great acidity and gradual wasting. He had no vomiting, but a good deal of diarrhœa, and died, gradually exhausted, after a year; the stools at last having become black and bloody. The stomach was found adhering both to the colon and the diaphragm. At the part of adhesion to the diaphragm, a portion of the stomach was destroyed by ulceration; and by means of the adhesion, a part of the diaphragm supplied the place of the part that was destroyed. A woman, mentioned by Pinel, had laborious digestion, pain of the stomach after eating, and gradual wasting. She had nausea, but very seldom vomited, and died after several months; for a short time before death, the vomiting had been rather more frequent. The stomach adhered to the liver and to the pancreas. The mucous membrane was irregularly destroyed and ulcerated. At the point of adhesion to the liver, the parietes were perforated by ulceration. The pyloric extremity was considerably thickened; the omentum was thickened and indurated.

In this insidious and dangerous affection, the symptoms appear to vary exceedingly, according to the extent of the part that is affected; and this seems to admit of remarkable varieties. In a case by Dr Carmichael Smyth, * it was of wonderfully small extent, and the symptoms were so slight, that they had scarcely been attended to, till the fatal event took place unexpectedly. A young lady, aged 15, had for many months complained occasionally of pain at the stomach, but so slight that no attention had been paid to it, until one evening she was seized with violent pain and vomiting, and died in 24 hours with symptoms of peritoneal inflammation. On dissection, extensive peritoneal

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* Medical Communications, Vol. II. p. 467.

inflammation was met with, and considerable effusion of turbid fluid in the cavity of the abdomen. In the anterior part of the stomach, there was a round ulcer, no larger than a sixpence, with hard callous edges; and the coats of the stomach at the seat of it were considerably thickened. On minute examination, it was found that the ulcer had entirely penetrated the stomach by an opening sufficient to admit a quill. The other parts of the stomach were healthy.

In this remarkable case, there is every reason to suppose that the fatal attack had been connected with the perforation of the stomach, and the escape of its contents into the cavity of the peritoneum. Many cases of the same kind are on record, differing in their previous history, but presenting similar symptoms in the fatal attack. M. Gerard has collected about seventeen examples of it, in a Memoir " *Des Perforations Spontanées de l'Estomac.*" In some of these cases, there had previously been chronic vomiting, and other symptoms indicating serious disease in the stomach; but in others, the previous symptoms were slight and obscure, and some had enjoyed tolerably good health. The fatal symptoms commenced with a sudden attack of most acute pain in the epigastric region, afterwards extending over the abdomen—sometimes, but not always, with vomiting. The pain continued with great anxiety, oppression, and every appearance of extreme suffering. The abdomen was, in some cases, distended and tender—in others contracted, and drawn into balls; and death took place, with rapid sinking, in periods of from twelve to twenty-four hours. On dissection, the stomach was found penetrated by ulceration of various extent; and articles of food and drink were found in the cavity of the peritoneum. The perforation seemed, in general, to be connected with ulceration of some standing, as appeared from its thickened and retorted edges, with thickening of the coats of the stomach, and adhesion to the neighbouring parts; and, in some cases, there was extensive organic disease at the part which had given way. In some of the cases, peritoneal inflammation had supervened; but in others, no morbid appearance was remarked among the intestines. Several cases of the same kind are described by Dr Crampton and Mr Travers, in the Medico-Chirurg. Trans., vol. viii. The following is the most remarkable case that has occurred to me; and it is chiefly important, from the slight and obscure nature of the previous symptoms.

CASE II.—A woman, aged 50, had been for several years in bad health:—her principal complaints were referred to the region of the uterus, and the os uteri was felt to be considerably indurated. She had also been liable to pain in the stomach, capricious appetite, and occasional vomiting; but these complaints had not been so severe or regular as to excite much attention, and had always been considered as of secondary importance, until she was suddenly seized with most violent pain, referred to a small spot in the epigastric region, accompanied by vomiting, and followed by severe pain and tension of the whole abdomen, great prostration of strength, and small, feeble, rapid pulse. She died in eighteen hours from the attack, the pain having continued unabated till a very short time before death. *Dissection.*—The stomach was healthy in its whole anterior aspect; but in the posterior part of it, there was a portion, about three inches in extent, which was indurated, and nearly half an inch in thickness. In the centre of this portion there was a round opening, about one-third of an inch in diameter, which entirely perforated the substance of the part. Internally, the opening was smooth, and rounded at the edges; and the smoothness extended to very near its external aperture, where a thin membrane had recently given way, so as to make the aperture ragged. The stomach, in other respects, was healthy. There was a good deal of fluid in the cavity of the abdomen, which seemed to have escaped from the stomach. There was scirrhus of the neck of the uterus, with considerable enlargement.

A lady, mentioned in the *Journal Géneral de Medécine* for August 1821, after having been dyspeptic for several years, was seized with severe pain, extending from the epigastric region towards the left kidney, accompanied by cold shivering, great anxiety, dyspnœa, and prostration of strength. The symptoms soon subsided, but returned on the following day, after taking food; and her principal distress was referred to a small circumscribed spot in the epigastric region. The attack returned in the same manner, after taking food, for four days, subsiding after some time, and leaving her in the interval free from pain. On the fifth day, the symptoms returned in the usual manner, but did not subside, and she died in twenty hours. On *dissection*, there was found peritoneal inflammation, with effusion in the abdomen, of a brown fluid, mixed with portions of food. The kidneys were natural. In the anterior part of the stomach, there was a round opening, nine lines in diameter. On the margin of it there was slight inflammation; but the stomach, in other respects, was healthy.—A woman, mentioned in the Medical Repository for March 1823, was liable to dyspeptic complaints, and was seized occasionally with pain in the stomach. It obliged her to sit down and rub her stomach with her hand for a few minutes, when it subsided, and she was able immediately to resume her work as a servant. One day, while engaged in her usual employment, she was seized with excruciating pain in the stomach, without vomiting or any other symptom. It continued through the whole day, and she died about midnight. On dissection, a viscid fluid, mixed with articles of food, was found in the abdomen. The stomach was perforated near the cardia, by an opening three-fourths of an inch in diameter internally, and one-third externally. The part was considerably thickened, and adhered to the liver.

To these observations may be added the remarkable case of Admiral Wassenaer, mentioned by Boerhaave, who died suddenly in the act of vomiting, or rather attempting to vomit, soon after he had dined. The lower part of the œsophagus had given way at the seat of an ulcer, and all the food and drink which he had taken at dinner were found in the cavity of the thorax.

Sudden rupture, subsequent upon chronic and often slight ulceration, occurs also in the intestinal canal, and with symptoms very similar to those now referred to in this affection of the stomach. For two interesting examples of this kind, presenting the affection in two very different forms, I am indebted to my friend Mr William Wood.

Case III.—A stout man, aged 36, who had previously enjoyed good health, was suddenly seized, while engaged at his usual employment, with violent pain in the abdomen, and vomiting. The pulse was not affected. The case was considered as ileus, and blood-letting and the other usual remedies were employed, but without benefit:—the symptoms continued, the pain extending over the whole abdomen—the pulse became quick and feeble, with rapid sinking of the vital powers, and he died in sixteen or eighteen hours. On dissection, nothing could be discovered in the cavity of the abdomen, except a considerable quantity of a thin feculent fluid; and it was only after a long and minute examination that a small opening was discovered in the lower part of the duodenum, capable of transmitting a large quill. It had its origin internally, in an ulcer of the mucous membrane, which, though of small extent, was considerably larger than the perforation.

The following case, by the same able practitioner, is still more remarkable, from the extent of organic disease which was met with, the patient having previously enjoyed good health, except habitual costiveness.

Case IV.—A gentleman, aged 60, on 6th December 1810, was sitting, after dinner, reading aloud to some friends who were with him, when he suddenly complained of most violent pain at the lower part of the abdomen, immediately above the os pubis, and vomited repeatedly. His countenance became pale, and his hands rather cold; and his pulse, when Mr Wood saw him, was feeble, and not frequent. The most judicious practice was employed without benefit;—the pain continued—the vomiting recurred at intervals—the pulse became very frequent and feeble, with rapid sinking of the vital powers,—and he died in about six hours. On *dissection,* much feculent matter was found in the cavity of the abdomen, which was found to have escaped through a perforation of the colon, at its lower part, a little above its junction with the rectum. The opening was larger than a shilling, and was surrounded by a mass of induration; and, for several inches, the intestine was hard and thickened, and, on its internal surface, extensively ulcerated. At the lower part of this diseased portion, from two to three inches below the rupture, the intestine was contracted by a hard ring, so as scarcely to transmit a finger.

The remarkable feature in these cases, is the perforation having taken place without any previous illness. I have formerly described several cases in which such perforation took place after symptoms, which had distinctly indicated disease of the mucous membrane of the bowels.* The affection appears in a third form, occurring in the course of a febrile disease, without symptoms which had previously indicated disease in the bowels. Many cases of this kind are mentioned by the French pathologists. In the *Archives Génerales de Medécine* for January 1823, M. Louis has described some of such interest, that I shall make no apology for introducing a report of them, as an Appendix to this paper. (*See* Foreign Medical Intelligence.)

On the other hand, it is remarkable what extent of disease may exist in the stomach, and yet the disease be drawn out to a great length, and with slight and obscure symptoms. A woman mentioned by Pinel received an injury on the stomach, from a thrust by the head of a goat, followed by tenderness of the epigastric region, which continued without any other symptom for two years. She then had more fixed pain, and a tumour appeared, which was tender to the touch. At first it was two inches in diameter, but soon increased to double that size. After a few days this tumour suddenly disappeared, during a fit of coughing, leaving only a tenderness of the epigastric region. She then had cold shiverings, followed by heat, great weakness and emaciation, and death, after four months from the time when the tumour appeared. For some time before death, the pain of the stomach had been more acute, with difficult breathing, hiccup, and fetid eructation; but no other remarkable interruption of the functions of the stomach. The appetite had continued tolerably good through the whole course of the disease, and the bowels regular, except immediately after the disappearance of the tumour, when there was diarrhœa. On *dissection*, the intestines were found inflamed, and adhering extensively to the omentum, and to the parietes of the abdomen. The stomach adhered to the liver and to the pancreas, at the small curvature; the coats were an inch in thickness, and the large curvature adhered to the colon. The mucous membrane of the stomach was covered by a fetid puriform matter; and a burrowing cavity was found, which admitted the finger, and conducted it to the part on the small curvature where there was the adhesion to the liver. There was also a communication betwixt the stomach and the ileum.

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* Edin. Med. Journal, Vol. XVI. p. 177 and 329.

In this remarkable case, there had evidently been most extensive inflammatory action, and a collection of matter had probably taken place betwixt the coats of the stomach, at the place where the burrowing cavity was met with. It had probably formed the tumour which was felt externally, and disappeared so suddenly, the abscess bursting into the stomach, and the matter being discharged by the bowels.

In most of the cases now referred to, the disease seems to have been originally simple inflammation, in a very chronic form, terminating by its usual consequences—thickening, adhesion, and chronic ulceration. But another modification of the affection occurs in the same parts, and with nearly the same symptoms, under the form of tubercular disease, with the unhealthy ulceration which usually results from it. A woman mentioned by Morgagni, was affected with pain of the stomach for several years. She then had loss of appetite, nausea, fever, and vomiting of blood. The epigastrium was tender to the touch, but no hardness could be discovered. These attacks occurred frequently, and were relieved by bleeding; and several times she seemed to be getting well. A tumour then appeared above each clavicle, with difficult breathing and severe headach. She still had uneasiness in the stomach, but no return of the vomiting of blood, and died, gradually exhausted. On dissection, the coats of the stomach were found much thickened, and very soft. Its cavity contained a considerable quantity of a fetid pultacious matter, which seemed to have been discharged from the posterior part of it, where the coats were so much thickened, as to form a tumour projecting into the cavity. This tumour was ulcerated and gangrenous, and resembled a ruptured abscess of the most unhealthy kind. The pancreas was hardened; the other parts in the abdomen were sound. The tumours above the clavicles were masses of diseased glands.

In the following case, again, the disease had more the appearance of the cancerous ulceration. It is the most remarkable example that has occurred to me of destruction of the stomach by ulceration.

Case V.—A lady aged 49, had been in bad health through the winter 1811–12, complaining chiefly of general weakness, and a constant uneasiness across the epigastric region, with occasional attacks of acute pain towards the left side. In May 1812, she began to be affected with vomiting, which continued from that time, and became more and more urgent. I saw her in July, and found her much emaciated and reduced in strength, so as to be confined to bed. She complained of an obtuse pain in the epigastric region, where considerable hardness was felt; and she vomited a portion of every thing she took, sometimes immediately after taking it, and sometimes a considerable time after. The pulse was weak, and rather frequent. She continued with very little change till the beginning of September, when the vomiting subsided, and she was completely free from it for more than a fortnight. During this time she was affected with diarrhœa; her strength sunk, and she died on the 23d, the vomiting having returned three or four days before her death, though with less severity than before. During the period when she was free from vomiting, she took food and drink of various kinds, and in very considerable quantities, and continued to do so till a few hours before her death. *Dissection.*—On opening the abdomen, and looking for the stomach, a large irregular opening presented itself, which was found to lead into the cavity of the stomach, a large extent of the great arch being entirely destroyed. On the left side, in the region of the spleen, there was found a large irregular mass, which appeared to consist of an enlarged and diseased spleen, and the remains of the great arch of the stomach, so blended into one mass, that it was impossible to distinguish one part from another. In the substance of it there was a cyst full of very fetid matter. This mass was attached to the cardia by a narrow neck, which remained of the coats of the stomach at that place; and when the parts were taken out and displayed by suspending the stomach by the cardia and the pylorus, the appearances were very remarkable. When stretched out in this manner, about one half of the stomach, at the pyloric extremity, was sound and healthy. This portion was attached to the cardia by a narrow portion of the small curvature which remained; and, by another small portion of the great curvature, the mass now referred to hung down on the left side. The remainder of the stomach consisting of the left side and lower part of the great arch, was entirely wanting, to such an extent, that when the parts were extended in the manner now mentioned, it appeared that nearly one half of the stomach had been destroyed. There was reason to believe, that the part which seemed to be wanting was involved in the diseased mass on the left side, and that the sound parts had been separated from this portion, by a line of ulceration of such extent, that the sound extremity remained attached to the cardia only by a portion about two inches in breadth, which remained of the small arch. The ulcerated edge, where the separation had taken place, was studded with numerous hard tubercles, like the edges of a cancerous ulcer. The pancreas was hard, the liver pale and soft, the other viscera were healthy.

Under the various forms illustrated by the cases which have been referred to, this dangerous affection of the stomach may be going on, with much diversity in the symptoms. They may be severe, and indicating serious disease, or they may be such as, without very great attention, are likely to be considered as merely dyspeptic. There may be hardness in the region of the stomach, or there may be nothing to be discovered by the most careful examination—and it is, in fact, extremely difficult to propose any rules by which the disease can be distinguished in its earlier stages. It may be suspected, when there is either permanent uneasiness in the region of the stomach, or pain recurring with regularity after meals, not prevented by every attention to the quality of the articles taken—when there is tenderness on pressure, especially if the pain and tenderness be always referred to a particular spot of small extent, and distinctly defined—when, along with these symptoms, there is vomiting occurring at short periods after meals, and after articles of the mildest quality—and when, without any very urgent or defined symptoms, a patient with affections of the stomach is becoming progressively weakened and emaciated, in a manner which his symptoms, if considered as merely dyspeptic, could not account for. It must be confessed, however, that none of these symptoms are entirely to be relied on--that most or all of them may exist in connection with a state of the stomach which is merely dyspeptic--and that, under a more serious form, they may even go on to a fatal termination, with every appearance of extensive disease, and yet nothing be discovered by the most careful examination. A woman mentioned by Chardel had been affected, for eight months, with habitual vomiting, every article of food being brought up soon after it was taken. She was then suddenly seized with violent pain of the stomach, followed by copious vomiting of blood, and died in fifteen minutes. The stomach was found full of coagulated blood, but no disease could be discovered in its structure, or in any of the neighbouring parts; and even the source of the blood could not be ascertained. In another case, also of long standing, in which there were frequent attacks of vomiting of blood, and death by gradual exhaustion, he could detect nothing but some varicose veins upon the mucous membrane of the stomach.

Upon the whole, it appears that this important part of pathology is still involved in much obscurity; but it is of much importance to be kept in mind, as an established principle, that symptoms which, at first sight, appear to be merely dyspeptic, often depend upon chronic inflammation of the stomach. Where the diagnosis is so difficult, indeed, the practice must be often empirical; but, when dyspeptic symptoms are unusually severe, or unusually obstinate, or are accompanied by an unusual degree of emaciation or debility, we ought always to suspect the existence of some more deep-seated cause, than that condition of the stomach which we usually understand by the indefinite term Dyspepsia. The treatment, in such cases, must be regulated by the period of the disease, and the activity of the symptoms. They will probably seldom admit of general bleeding; but much benefit is obtained from topical bleeding, blistering, issues, and the tartar-emetic ointment. The food should be of the mildest kind, and in very small quantity, with abstinence from all stimuli, and from bodily exertion. Hence the importance of distinguishing the affection from dyspepsia, whenever the distinction can be made, as the regimen and exercise proper in dyspeptic cases would probably be injurious.

Whether the disease can be cured after it has advanced to the stage of ulceration, may perhaps be doubted; and it cannot be easily determined; because, when a case terminates favourably, we have no means of ascertaining that ulceration really existed. From what we observe, however, in regard to the intestinal canal, we have good reason to believe that ulceration there may terminate favourably, provided it be simple ulceration of the mucous membrane, without tubercular disease, or much thickening of the coats of the intestine. I think I have seen the cicatrices of such ulcers, when the patient has died of another disease, after having been for a considerable time free from every complaint of the bowels. It is probable that simple ulceration of the stomach may admit of the same favourable termination; and we certainly see cases get well, in which, from the violence and duration of the symptoms, we had good reason to suppose the existence of that disease. Of the various remedies that are employed in such cases, it is not easy to say which are really beneficial. Mercury, in small quantities, seems to be useful in such cases—in others, lime-water, and the oxyd of bismuth; but the remedy from which I have seen the most decided benefit, in cases in which I have suspected the existence of that affection, is the sulphat of iron. The following case will serve as an example of the affection which I here refer to.

A lady, aged about 30, came to Edinburgh from a distant part of the kingdom, in summer 1818. She was affected with a violent pain of the stomach, which seized her every day, immediately after dinner, continued with great violence through the whole evening, and gradually subsided about midnight, or soon after it. It sometimes attacked her after breakfast, but more rarely. The complaint was of two years' standing, during which time, great variety of practice, and every variety of diet, had been tried, with but very slight and transient benefit. The paroxysms occurred with perfect regularity; and she was considerably reduced in flesh and strength. In the epigastric region, no swelling could be discovered; but there was considerable tenderness on pressure at a particular spot. She had a sallow unhealthy look; and her whole appearance gave strong ground for suspecting organic disease. Various remedies were employed during the summer, with little advantage. At last she appeared to derive some benefit from lime-water, and returned home in autumn 1818, rather better. But the affection soon recurred; and she returned to Edinburgh in 1819, as bad as ever. After another trial of various remedies, this severe and untractable affection subsided under the use of the simple remedy to which I have referred. She took gr. ij. of sulphat of iron three times a day, with a few grains of aromatic powder; and with the addition of a small quantity of aloes, so as to regulate the bowels. Under the use of this remedy, she was very soon free from complaint, and has continued to enjoy good health.

There are other affections which seem to be connected with inflammatory action of the mucous membrane of the stomach, but I shall only allude to one of them. A woman, aged about 30, and previously healthy, in the course of a slight febrile disorder, became affected with pain and tenderness of the epigastric region, extending over the abdomen. The mildest articles of food produced great pain. There was diarrhœa, with much griping and frequent vomiting. The affection was accompanied by a very feeble pulse, great debility, and collapse of the features; and there was a peculiar rawness and tenderness of the tongue, mouth, and throat. After various remedies had been tried without benefit, these symptoms subsided speedily under the use of lime-water.

[*To be continued.*]

II.

Case of Subclavian Aneurism. By J. H. Wishart, F. R. S. E. Surgeon in Ordinary to his Majesty in Scotland.

John Robertson, æt. 47, a porter, was admitted into the Royal Infirmary of Edinburgh on the 7th August last. He complained of considerable pain of the left elbow-joint, particularly on motion. There was no perceptible swelling or discoloration of the soft parts, but he was able to make very little use of the arm, and he complained of great weakness of it. The motions of the fore-arm performed freely, and without pain; general health good; reports, that about six weeks ago, was attacked with pain in the elbow-joint, which has since increased, though local bleeding, blisters, &c. have been frequently employed by the surgeons of the Leith Dispensary, where he was an out-patient.

From his admission, to the 20th August, the patient referred all his complaints to the elbow-joint, and the fleshy part of the biceps muscle. At this time the fore-arm became pained and œdematous; a roller was directed to be applied from the fingers upwards. On the 21st, he said that there was a good deal of swelling of his side, which was found to be œdematous nearly as low down as the spine of the ilium. When his shirt-neck was opened, a considerable swelling was perceived under the pectoral muscle, extending along the lower margin of the clavicle to the axilla. On pressing on this tumour, a distinct pulsation was felt in it, and a peculiar thrilling sensation, more especially at the sternal extremity of the tumour. The pulse at the wrist was observed to be much feebler than the pulse of the other arm.

On questioning the patient, he stated, that about three weeks before his admission, he had noticed a small knot like a bean under the edge of the clavicle, but he could not point out the precise spot; and he said that he had paid no attention to it, as it gave him no uneasiness. He did not conceive that it was the cause of the pain in his arm, and he had never mentioned it to any of his medical attendants.

On the 22d, no pulsation could be felt in the tumour; but towards the sternum the peculiar thrilling sensation was still perceived, though not so distinctly. No pulsation could be felt at the wrist, nor in the course of the humeral artery. The pulse in the other arm was natural; the pulsation of the left subclavian artery was felt distinctly above the clavicle.

As there was some obscurity in the history of this case, and as the patient stated that he had been seen by Dr Kellie of Leith, it was resolved to wait till the following day, when an opportunity might be had of obtaining the assistance of that eminent surgeon. On the 23d, Dr Kellie visited the patient along with the surgeons of the Hospital, and the nature of the case was now decided. No pulse could be felt in the arm; the œdema had increased; the tumour below the clavicle was more prominent; the pulsation in it distinct. It was then resolved to tie the subclavian artery, and I immediately performed the operation in the following manner.

The patient was placed on the operating table, with his head and shoulders considerably elevated; the head was turned to the right side, and the left shoulder rather depressed. An incision was then made about three inches in length, beginning near to the clavicular attachment of the sterno-mastoid muscle, and carried about half an inch above the clavicle, in the direction of that bone, towards the shoulder. After dividing the integuments, the *platysma myoides* muscle was brought into view, which was divided the whole length of the external incision; two small arteries were cut at the outer angle of the incision, which were immediately secured by the tenaculum. I then pushed my fingers down in the direction of the artery, and, with them and the handle of the knife, I separated the cellular substance covering it. Two cords of nerves were first brought into view; they were drawn a little towards the shoulder with a blunt hook; the scalenus muscle was felt; and at its outer margin, and a little under its edge, the subclavian artery was felt pulsating under the finger. It was very slightly detached, and a ligature passed under it with the common aneurismal needle. On compressing the artery between the two ends of the ligature, the pulsation of the tumour was stopped, and the ligature was tied without any difficulty, the two fore fingers being pressed down upon the artery while the ligature was tightened, which was secured with a double knot. The wound was then brought together with three stitches, and strips of adhesive plaster. The patient bore the operation well; he was affected with slight cough during the separation of the parts covering the artery; but he expressed no uneasiness when the ligature was tightened, and did not lose above an ounce or two of blood from the small vessels. He was put to bed, his arm was laid on a pillow, and bent over his breast. After the operation, the arm felt a little benumbed and cold; it was wrapt in flannel, after which it regained its natural heat. Pulse in the evening 100, of natural strength.

Bibat pot. tartarisat. ad libit. Haust. anod. h. s.

24th.—Has passed a very good night; size of the tumour diminished; œdema of the hand and fore-arm much reduced; the left arm feels warmer than the right; but by the thermometer, the heat in the axilla is the same, 98°. Pulse 108, of moderate strength; tongue rather dry; some thirst; no stool. *Wishes to have small beer for drink; tea morning and evening.*

Pil. coloc. comp. vespere. Haust. anod. h. s.

25th.—Passed rather an indifferent night; free from pain, except at the left elbow-joint. Pulse 100; tongue loaded, but moist; heat 97; cough troublesome.

Mistur. mucil. cum tr. opii, urgent. tussi. Pil. coloc. comp. ii. statim. Enem. domest. si opus.

26th.—Cough relieved; a good night; pulse 108, regular, and rather full; bowels relieved by the injection last night.

Pulv. jalap. c. ℈iv. statim.

Noon.—Dressings removed; slight redness around the wound; stitches cut out; still complains of numbness of the hand and fingers; œdema of fore-arm gone.

Cataplasm. emol. vuln.

From this date, every thing went on favourably for some days; the discharge was moderate and healthy; and on the 29th, slight pulsation was felt in the brachial artery; but on the 1st September he was affected with nausea, loss of appetite, foul tongue, heat of skin, and flushing of face; pulse 108, full and hard.

Venesectio ad ℥xvi. Pulv. jalap. comp. ʒi. cr. m.

September 2d.—A tolerable night; blood very sizy and cupped; tongue foul; pulse 106, of moderate strength; cough very troublesome; countenance natural; skin cooler; medicine has not operated.

Post hor. duas injic. enem. si opus sit.

Vespere.—Pulse 100; fuller than in the morning; skin hot; medicine operated once; tongue dry.

Ft. V. S. ad ℥xii.

3d.—Felt relief from the bleeding; a good night; blood sizy and cupped; tongue foul, but moist; pulse 110, of moderate strength; heat 97; cough easier; slight pain of tumour increased on pressure, and on coughing; skin cool; no thirst.

Appl. solut. mur. ammon. tumor.

Vespere.—Pain of tumour rather diminished since the application of the lotion; pulse 120, rather hard; no stool to-day.

Mittat sang. e brachio ad ℥x. Injic. stat. enem. purgans, et cr. m. sumat. pulv. jalap. comp. ʒi.

4th.—Blood drawn last night cupped and buffy; two copious evacuations after the injection; has passed a restless night; pain in the aneurismal tumour rather diminished, but an erysipelatous redness, which was observable on its surface last night, has rather increased, extending down the side, with somewhat of a livid appearance; the small ligatures have come away, and the wound looks healthy; left arm of the natural temperature, and the swelling of the limb reduced. Pulse not yet perceptible at the wrist; pulse in the right arm 104, much softer. Tongue foul, but moist; no thirst.

Hab. pulv. jalap. c. ℈ii. statim. Cont. sol. mur. ammon.
℞ *Mistur. salin.* ℥ *viij. Tinct. digital. gtt. xl. M. Cap.* ℥*i.*
secunda quaque hora. Haust. anod. cum gtts. xxxv. h. s.

The patient convalescent. On the 7th, the sixteenth day from the operation, the ligature on the subclavian artery was found lying loose in the wound, and was removed. On the 8th, the pulse was distinctly perceptible at the wrist; appetite returning. The wound was healing rapidly; but the erysipelatous affection of the side, though faded in colour, was evidently tending to suppuration, and it burst during the night of the 12th, under the axilla, and discharged a quantity of matter of a grumous appearance. The opening was afterwards enlarged with a lancet; and on the 15th it was necessary to make another opening lower down in the side, to allow a more depending exit to the discharge which was very profuse. The aneurismal tumour was now gone, and the left clavicle as prominent as the right; the cellular membrane in the neighbourhood of the abscess was sloughy, and several portions of it required to be removed.

It was now thought of the utmost importance that the patient should be taken to a purer air than that of the hospital, and a lodging was therefore hired for him in the suburbs. In the course of a few days, this change effected a very perceptible improvement in every respect; his appetite returned, he was able to take animal food, wine and porter; and though matter again collected, and required to be discharged by the lancet, nothing material occurred; but his recovery was very slow, as might be expected from the quantity of blood that was requisite to be taken to subdue the attack of erysipelas, an occurrence which has, in many instances, retarded the cure of patients operated on in this city.

24th October.—The patient is now able to walk about. The arm is very much reduced, but he has perfect feeling in it. He is still unable to bend his fingers, and the pulse at the wrist is feeble. His appetite and spirits are good.

This case may now be considered to be brought to a favourable termination. The success of the operation, and the short time in which it was performed, may be ascribed to the method employed, viz. after making the external incision, and dividing the platysma-myoides muscle, the laying aside the knife and using the fingers in separating the cellular substance, so as to expose the artery. No difficulty was met with in passing the ligature, or drawing it sufficiently tight with the fingers; and had it not been for the severe erysipelatous affection, the patient would have been completely recovered in three weeks.

Having been obliged to leave town about the beginning of September, I was indebted to Dr Ballingall for his unwearied attention and skilful treatment of the patient during my absence; and by his advice he was removed to the country before my return.

York Place, 6th November, 1823.

III.

Successful Case of Amputation at the Hip-joint. By JAMES SYME, Member of the Royal College of Surgeons of London and Edinburgh, and Lecturer on Anatomy in Edinburgh.

It was in the summer of the year 1821, about the month of August, to the best of my recollection, that I first saw William Fraser, the subject of the following case.

He was then nearly seventeen years of age, though apparently much younger—being of a thin pale habit and countenance, without any the least symptom of puberty.

His complaint was a painful swelling of the left thigh, extending from the condyles to within a few inches of the trochanters. The skin was tense, smooth, and of a nearly natural colour; but the consistence of the limb itself was exceedingly hard and unyielding—so much so indeed, that it felt as if entirely composed of bone.

A little above the knee, on the outer side of the thigh, there was an opening discharging thin fetid sanies, which led to a deep winding sinus, that allowed the probe to pass inwards and backwards as far as the bone, which was found to be exposed and rough on its surface, a little above the ham, between the external and internal flexors of the leg.

I was told that the swelling had existed for several weeks, and that, at its commencement, the patient had complained of most excruciating pain, which had gradually become less violent, though it still continued so troublesome as to require frequent use of laudanum. The opening above mentioned, had been made recently by a surgeon, for the purpose of evacuating the contents of a very painful abscess.

No exciting cause of disease could be fixed upon, unless perhaps over-exertions at work (turning a wheel with the foot) or exposure to cold and wet.

Judging from all of these circumstances concerning the actual state and history of the case, I felt satisfied as to the nature of the disease; and gave it, as my decided opinion, that the bone being extensively diseased, while the appearances of reaction were very inconsiderable, amputation would in all probability be necessary for the patient's relief;—at the same time recommending, that though the operation ought not to be delayed after his health began to sink, every means should be used in the mean time to uphold the vigour of the system, and enable it to cast off the disease. With this view he went to the country, but returned in a few weeks,—if not altered to the worse, certainly not the least improved.

His health appearing soon afterwards to yield considerably, the operation was recommended rather strongly, but without the effect of persuasion; indeed the father and mother, though sensible people and affectionate parents, would not press a measure, which seemed so frightful to the patient, that he declared his firm determination to die rather than submit to it.

Seeing him in this mind, I contented myself with calling now and then—twice in the month or so—merely for the satisfaction of the poor boy, and that of my own curiosity as to the termination of so severe a case of a disease (*Necrosis*), which, for some time past, has much interested my attention.

I continued these visits for a long while, and had much reason to admire the strength of constitution which withstood the destructive effects of a discharge so profuse and long continued —to say nothing of the constant uneasiness and occasional acute suffering caused by the disease. But, at last, finding that the state of matters continued altogether without change, the young man performing all his bodily functions in a healthy manner, and so as to bear up against his inveterate enemy without seeming to advance or retreat a single step for more than a year, I grew remiss in my attendance, and at last lost sight of poor Fraser entirely.

I had not seen or heard of him for many months, when, one day in the latter part of August last, his father stopped me in the street, and told me that his son had now made up his mind to the amputation. I called soon afterwards, and found the patient fully resolved on being delivered from a disease which had confined him to bed for more than two years, and was daily getting worse and worse. I therefore subjected the limb to a very careful examination.

A considerable change had taken place since the last occasion on which I saw it. The swelling was greater than ever, and now extended to within an inch of the trochanter major. A profuse glairy discharge was poured out from many openings on the posterior and outer sides of the limb, and abscesses were constantly forming farther and farther up towards the joint. When I saw him, there was one which had just opened near the tuberosity of the ischium, and another was beginning to point not far from the trochanter major.

His appearance was also altered for the worse, being still more emaciated; and though five feet eight inches in length, he still retained the character of a boy.

His mother told me that she now observed a daily diminution of strength—that every evening he was very feverish, and that he got little or no sleep, owing to pain of the limb. The pulse, as it had been all along, was small and very rapid.

Notwithstanding all these unfavourable circumstances, I still considered the operation advisable, since the lad's destruction was otherwise inevitable; and this means afforded him a chance, which was rendered better by his own good spirit and thorough determination to live. His digestive functions continued tolerably healthy; and Dr Abercrombie, who had the goodness to visit the patient, in whom he took an interest, gave me still farther encouragement by coinciding as to the propriety of operating.

The next question was, At what part of the limb should amputation be performed?

In the earlier periods of the disease, nothing forbade removal below the trochanters; but now the case was very different.

I have said that the swelling could be traced distinctly to within an inch of the trochanter major, consequently it must reach the trochanter minor;—and every anatomist knows, that if the bone were cut higher than this latter process, the capsular ligament must be opened. Besides, granting that it was possible, by sawing obliquely through the spongy tissue which constitutes the bone between the trochanters, to remove all the sensibly swelled part without injuring the joint, still, considering the duration and spreading disposition of the disease, it was not probable,—I should rather say it was not possible,—that any portion of the bone could be sound or disposed for healthy action; and even if this difficulty could be got over, there would still remain another of equal magnitude—I mean the formation of sufficient flaps.

Plenty of skin might be got; but it was not likely that the thin, exhausted and distended muscles of the thigh would show much inclination for union, even if they could be secured in sufficient quantity to cover the bone;—and this the most hasty examination was sufficient to prove altogether out of the question.

Amputation at the hip-joint then appeared to me the only justifiable operation—an opinion with which my friend Mr Liston entirely agreed.

I knew very well that this operation had rarely or never succeeded in private practice; but that was no reason why it should not do so. * In the operation itself, I could see nothing to forbid recovery. Indeed, so far from regarding the acetabulum as any great obstacle to union, I considered the presence of a healthy cartilaginous surface as a much less evil than that of a great bone lacerated by the saw.

In determining as to the plan of operation, I had no hesitation in fixing upon the method of Lisfranc, since this was not only the speediest manner of proceeding, but also one which I had frequently practised on the subject, not only in Paris, under the direction of Lisfranc himself, but also at home in my own dissecting room.

On the 2d of September, assisted by my much esteemed and highly respected friend and instructor Mr Liston, and in the presence of Dr Abercrombie, Dr Anderson of Leith, Dr Scott, and Mr Marshall, Surgeon to the Forces, I performed the operation in the manner following.

Having, with some difficulty, placed the patient upon a table so that the affected limb was perfectly free, and ascertained that Mr Liston was ready to make pressure when and where required, I introduced a narrow knife, about a foot long in the blade, which was sharp on one edge only, at the proper place for transfixing the limb; but being prevented by the bent position in which, owing to long habit, the patient obstinately retained it from passing onwards in the direction of the tuberosity of the ischium by the neck of the femur, I lost no time in the repetition of fruitless attempts, but instantly changed my plan.

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* When Mr Liston, two years ago, tied the subclavian artery with success for the first time in Great Britain, that operation was looked upon as desperate as amputation at the hip-joint is now. But since then, instances of its success have become quite common in the records of surgery—Mr Todd of Dublin, Mr Bullen of Lynn, and Mr Wishart of Edinburgh having all met with fortunate cases.

Without removing the point of the knife, I brought down its edge obliquely, and, by a sawing motion, quickly cut back, in a semicircular direction, to the tuberosity of the ischium, up along the femur, and round the trochanter major, so as to form very speedily identically the same flap which would have resulted from the plan I meant to have followed.

While Mr Liston covered the numerous cut arteries with his left hand, and compressed the femoral in the groin by means of his right, I gathered together all the mass of undivided parts on the inner side of the thigh with my left hand, and then insulated the neck of the bone by passing the knife close past its lower surface. I now cut close down along the bone for some way below the trochanter minor, and lastly made my way outwards obliquely, so as to form a good internal flap.

Mr Liston holding aside the flaps, I made a single cut with my long knife upon the head of the bone, which started with a loud report from its socket as soon as abduction was performed. Finally, I passed the knife round the head of the bone, cut the triangular and remaining portion of capsular ligament; and thus completed the operation, which certainly did not occupy, *at the most*, more than a minute.

I then proceeded, without delay, to take up the arteries, which were tied by our very promising pupil Mr Thomas Evans.

As soon as the femoral, which had been completely commanded by pressure in the groin, was secured, Mr Liston relaxed his hands in order that we might form some estimate as to the size and number of bleeding vessels; and then, had it not been for thorough seasoning in scenes of dreadful hemorrhage, I certainly should have been startled, prepared as I was to expect unusual vascularity, owing to the extensive action so long carried on in the limb.

It seemed indeed, at first sight, as if the vessels which supplied so many large and crossing jets of arterial blood could never all be closed. It may be imagined that we did not spend much time in admiring this alarming spectacle. A single instant was sufficient to convince us, that the patient's safety required all our expedition; and, in the course of a few minutes, hemorrhage was effectually restrained by the application of ten or twelve ligatures.

The flaps were now brought together, and retained in contact by means of five or six stitches. Some dry caddis was laid over the wound; and, lastly, I applied a single-headed roller obliquely round the body and stump, moderately tight, so as to afford proper support to the flaps; and then we lifted into bed the patient, who was wonderfully little exhausted.

The operation was performed at twelve o'clock; and, during the remainder of the day, nothing occurred of much interest except occasional vomiting, which I thought, at the time, might be attributed to the effects of an opiate given about two o'clock, on account of pain and restlessness.

In the evening, owing, as it seemed, to the exertion of vomiting, some bleeding appeared, but soon ceased. Not long afterwards, however, the patient complained so much of pain and tightness in the stump, that I was induced to cut the bandage, and remove two of the stitches. This allowed some clotted blood to escape, and gave great relief. The wound was then dressed as before.

I heard no more of the patient that night, as I went to bed early on account of indisposition, under which I had been labouring for several days; but Mr Liston saw him at twelve. He was then very low, his foot being cold, and his pulse hardly perceptible. He gradually recovered, however, under the use of moderate stimulants; and next morning, his state, as far as regarded strength, was better than it had been since the operation. The vomiting still continued, and, I was told by the young gentlemen who sat up, it had been, through the night, very frequent and distressing, hardly leaving him at rest for more than a quarter of an hour. I contented myself with directing that he should take some well-seasoned beef-tea, often and in small quantities.

In the forenoon, finding that the stomach still continued so extremely irritable as to reject instantly whatever was introduced into it, I prescribed an injection, with forty drops of laudanum.

Dr Abercrombie had the kindness to visit the patient, soon afterwards, at my request, and recommended persistence in the use of opiates, both solid and fluid. The good effects of this practice were soon manifest, the vomiting gradually subsiding, and the stomach beginning to retain beef-tea, and other fluids given in small quantity. The injection was repeated in the evening, and a grain of solid opium continued to be given by the mouth every three or four hours.

Next day, that is, the second after the operation, there was a decided improvement in all respects, the patient having slept well, and being almost free from vomiting. But a new, distressing, and, at first sight, alarming symptom, soon began to make its appearance in the form of a most violent hiccup. I was told, however, that he had been subject to this complaint before the operation, and therefore felt less uneasy on its account. In fact, it went away, by degrees, under the use of solid opium and peppermint lozenges, though attempts at returning were manifested for many days.

On the third day, the wound was examined, when it looked perfectly well. Some clots of blood came away with the dressings, and others presented themselves at the gap which was caused by removing the two stitches. These were not disturbed then; but, having become gradually detached, were thrown out at the succeeding dressings, with a quantity of thin serous discharge. Some slips of adhesive plaster were applied, where necessary, for retaining the edges of the wound in proximity; and the dressing in other respects continued to be of the lightest description.

The discharge, which was at first thin and watery, gradually became thick and more healthy. The opium was dispensed with after the first week. The bowels were brought into a good state by the occasional use of turpentine injections; and the patient's appetite, sleep and pulse, continued to improve. In short, every thing went on well.

About the 12th day, the ligatures began to separate, and, by the third week, they were all withdrawn.

Though the wound always retained a healthy and healing appearance, the patient suffered much from occasional returns of irritability in the intestinal canal, marked by diarrhœa and vomiting, which, it may be believed, tended greatly to protract recovery, and required assiduous medical attention. How much I am indebted to Dr Abercrombie for his kind advice and assistance on these occasions, as well as benevolent attention to the poor lad in other respects, it is impossible for me to express; and can be properly understood by those only who have been placed in circumstances of equal responsibility.

About a month after the operation, the wound being nearly healed, I was flattering myself with the prospect of soon seeing Fraser restored to health and strength, when, to my great regret and disappointment, symptoms of ascites made their appearance, and, notwithstanding all our endeavours to the contrary, by means of gentle diuretics, such as supertartrate of potass, and nitrous ether, which were all that his weak state could bear, continued to increase, and at last carried him off at the commencement of the eighth week from the operation.

Dr Abercrombie examined the body along with me. We found, in the cavity of the abdomen, a quantity of water sufficient to fill a large wash-hand basin. The liver was more than twice its usual size, very hard, yellow, and completely altered in its structure, there being no appearance of acini. The gall-bladder contained a quantity of fluid, more resembling serum than bile. The spleen was double the natural bulk, but sound and healthy.

The intestines were exceedingly thin, and looked as if destitute of a muscular coat. The lungs, heart, and all the other viscera, were, to appearance, perfectly sound.

In concluding the history of this case, I may be allowed to make the following remarks.

Amputation at the hip-joint is discountenanced by most surgeons, on the following grounds:

1. It is too great a shock for human strength to support.

2. The hemorrhage which must attend so great an operation, where no tourniquet can be applied, will render the patient's recovery, should he even get over the immediate shock, very tedious, if possible.

3. The confusion which must be occasioned in the circulation, by removing instantly so great a portion of the body, cannot but produce some immediate fatal disorder and congestion.

4. The extent of cartilage and synovial apparatus must prove an almost insuperable obstacle to union.

5. The mutilation inflicted renders life miserable, and not desirable.

6. The operation is never necessary.

In answer to the four first of these objections, I appeal to the case of William Fraser.

1. He was weak and emaciated to an extraordinary degree, yet he stood the shock well.

2. Notwithstanding the extreme vascularity of the limb, the hemorrhage did not exceed a few ounces.

3. Though the limb removed, owing to its having so long engrossed the attention of the system, may be regarded as equivalent to *half* of the body, no immediate disorder occurred in the circulation.

4. The wound healed kindly and soundly over the acetabulum.

If the 5th objection be good, then no surgeon need ever remove the thigh at any point in its upper third; for it is clear, that, unless this portion be left entire, no use can be made of the joint, and the patient must support the side of his body on a crutch, just as those people do who have a small and distorted limb. Indeed, I have often thought that these useless, dangling, inconvenient, unseemly appendages, would be much better off than on.

Lastly. As to the operation being never necessary, I shall only observe, that I myself have seen one other case in which it would have been most justly warranted. I allude to a compound-comminuted fracture of the thigh between the trochanters. The patient lived a month, and afforded two most favourable opportunities of performing the operation, viz. before the fatal suppuration was established, and after the system was in some measure reconciled to it. I have also heard of several met with by my friends; and as to Fraser's case, all who have seen the bone—and they are not few, or little entitled to respect—agree as to the necessity which there was for its *entire* removal. It is indeed thickened higher up than the trochanter minor, and evidently diseased throughout the neck.

I am no advocate for operations, whose only interest is their danger,—and assuredly regard the knife at all times as a great, though too often necessary, evil; yet I feel no hesitation in recommending, to the serious attention of operating surgeons, amputation at the hip-joint, although it be the greatest and bloodiest operation in surgery; for I am quite sure that there is sometimes no other mode of prolonging existence. And I firmly believe, that, if the operation be done properly, and above all quickly, its success will be general, if not uniform.

IV.

Remarks on Amputation. By James Syme, Member of the Royal College of Surgeons of London and Edinburgh, and Lecturer on Anatomy in Edinburgh.

When we consider the thickness and acute sensibility of the parts necessarily divided in the amputation of a limb, together with the size and number of blood-vessels cut across, as well as the extent and complicated nature of the resulting wound, it seems wonderful that any operation in surgery should be undertaken with more care or preparation. Yet the fact is, that while few surgeons venture on extracting the cataract, extirpating tonsils, opening the trachea, and other operations of little magnitude but dangerous execution, every tyro in our profession looks upon himself as fully qualified to dismember his fellow-creatures.

The explanation of this, at first sight, very unaccountable circumstance is to be found, I believe, in the common idea; that as soon as the wound inflicted by an operation is dressed and the patient has been put to bed, all the surgeon's responsibility is at an end; and that though he may claim the merit of success, Nature must bear the blame of all secondary bad consequences. For every surgeon who is not very expert will, as long as this opinion maintains its ground, naturally dread and avoid those operations which may be attended with bad or even fatal effects, owing to hemorrhage or injury of vital organs, before the patient is *off the table*, while he undertakes, without hesitation, those that cannot bring him into discredit by proving destructive, until what is considered the period of his own responsibility has elapsed, and Nature shall have become answerably for the result.

Of all such operations, there is no one performed with more safety, in the first instance, than amputation. Since there are here no parts essential to life which *can* be injured, and hemorrhage may be effectually restrained by the hard screwed tourniquet. It is accordingly, as I have said before, never declined even by the most ignorant practitioners; and students of surgery, who are well aware of this fact, which, with the candour and inexperience of youth, they attribute to the small share of dexterity required on the occasion, bestow very little of their attention on what is believed to be so simple a matter.

But it appears to me that, in the performance of every operation, the duty of the surgeon extends somewhat beyond merely removing the disease which renders his interference necessary, since, besides doing this safely and effectually, he ought, as I think, to make some allowance for Nature's share of the labour, and be very careful not to injure the remaining sound parts farther than is absolutely necessary.

Indeed I am quite satisfied, that nothing would tend so much to the general improvement of surgical practice, as a common understanding that average success is the surest criterion of operative skill; and that though the surgeon may justly feel pride and satisfaction when his efforts are successful, he must be contented to share the blame which is at present so liberally heaped on Nature when the result is different.

But of all operations, it is particularly to be desired that this way of thinking should obtain relative to amputation, where the happiness or misery, indeed the existence, of the patient depend so much upon the manner in which the soft parts are divided and left for reunion.

Unless the different parts of the stump be properly proportioned to each other, the worst consequences must follow, not only during the cure, but even if that should be completed, throughout the whole after life. For comfort requires something more than a covering of skin over the face of the stump, and demands that the bone should be well protected with flesh, which may form a soft elastic cushion sufficient to guard against the bad effects of cold, and prevent concussion from external violence, so as to supply the place of that curious mechanism which Nature has contrived for the same purpose, and placed in the articulations of the numerous bones composing the extremities. Moreover, if the very dissimilar tissues which are to constitute the future stump be not cut smoothly, evenly, and in such a way as to admit of being brought into mutual contact without having violence done to their natural affinities, the cure will, in all probability, be tedious and imperfect. And I may also remark, that where so many sensible parts must be cut, the surgeon who is longer in doing the operation than is necessary for its proper performance, will hardly fulfil his duty to the patient.

Having said enough to show that the manner of performing amputation is a subject of great importance, as well to the credit of surgery as the interests of humanity, I shall now inquire into the merits of the different methods of operating in common use, and endeavour to ascertain how far they are well calculated to answer the intentions with which they are employed.

And, first of all, I shall consider the plan of circular incision, so commonly followed in this country. The manner of proceeding here, as every surgeon knows, is, first, to dissect back the skin for some inches; then to cut the muscles as high as they are exposed; and, lastly, to remove the bone as far up as the retraction of the flesh will allow. Now, of all the operations ever adopted for amputation, I certainly look upon this one as the worst, inasmuch as it is tedious, painful, and little calculated for leaving the sound parts in a good state either for healing, or forming a proper covering for the bone.

That it is tedious, every one will allow who reflects upon the multiplicity of its *steps.* First, there is the cautious circular incision of the skin; the change of knife; the dissection of the skin; its *turning-back;* the change of knife; the adjustment of the assistant's fingers, that they may not be cut off by the relentless sweep which divides the muscles; the change of knife; the separation of muscles from the bone; the division of periosteum; the application of retractors; and, lastly, the sawing of the bone. It may be said that I have mentioned more steps than enough—no doubt; but I must describe the operation as it is generally performed.

But if it be granted that this is a tedious way of operating, then my second position follows as a matter of course; for if the pain of all operations be in the inverse ratio of their quickness, it is clear, that where so many sensible parts as must be cut in amputation are divided by slow and numerous stages, the patient's suffering cannot be but truly intolerable.

My third assertion regarding this plan of operation is the most important, as far as its credit is concerned. For I am aware that there are many patients, as well as surgeons, who esteem the time spent, and pain suffered in an operation, as of little consequence when compared with its proper performance—and, no doubt, with much reason, though I must be allowed to say, that, as far as my observation goes, the operations which are performed best are done *quickest*.

Let us see, then, in what condition the soft parts are left to unite, after an amputation performed, according to rule, by circular incision. And, for example, suppose we examine the appearances presented by the stump of a thigh which has been amputated after this fashion, before any bandages or other restraints have been put upon it. The first thing that strikes us is the bone standing eminently conspicuous, stripped of its periosteum and other connections; round it the muscles are to be seen, exposing an irregular ragged surface, owing to their unequal division and retraction; and, lastly, as our view is directed towards the circumference, we observe the thin, bloodless skin hanging loosely about the ill-assorted parts with which it will require to unite, when drawn into a bag by stitches or adhesive plaster.

The application of a bandage will, no doubt, improve the state of matters to a certain extent: it will compress the soft parts generally, and push the muscles forward so as to form a cushion for the bone; and if union between them should fortunately happen by the first intention, then a tolerable stump may be obtained; but such an occurrence is not very apt to happen where the raw edges are irregular, and *forced* into contact, and suppuration much more frequently takes place; the consequence of which is, that the muscles, not being prevented from obeying the tendency of their tonic contractility, by natural union over the bone, withdraw themselves from its extremity, which is consequently exposed to the air; and thus the patient is condemned to all the miseries of sugar-loaf stump, unless means of prevention be employed by nature in the way of exfoliation, or by the surgeon performing a secondary amputation.

If I ask one surgeon, or all the members of the profession, what is the chief requisite of a good stump? I shall be told, plenty of muscle; but if this be the case, I should wish to inquire of those who admire the plan of amputating by circular incision, how they reconcile their principles and practice? for surely there is no method more effectual than this for saving redundant skin, and less calculated for leaving the necessary quantity of flesh.

The skin not being possessed of much elasticity,—whereas the muscles not only are so, but besides have a constant tendency to become shorter, in consequence of their tonic power of contraction continuing to exert itself after the resistance naturally opposed to it has been removed,—we might naturally imagine that surgeons, who proceeded on the principle of saving the flesh long in proportion to the integuments, would cut the former as low as possible, and the latter as high; yet, on the contrary, we see the skin dissected back for some inches, so that connection with subjacent and contractile parts may have no effect in making up for its deficiency of elasticity, and so rendering it shorter, while the muscles are carefully cut as high up as they are exposed; and, as if this were not sufficient for diminishing their length, the connections which they have with the bone are separated, so as to allow the contractile power to exert its full effect; the consequence is, that the bone, immediately exposed to a great extent, deceives the surgeon, who, judging by the portion removed, thinks he goes high enough when sawing at the point to which the muscles have themselves retracted. Thus, every thing that can be done, to diminish the quantity of flesh, is done, and nothing remains for forming the stump but skin and bone. Whence it happens that a good covering can rarely or never be obtained for the bone, which is generally protected, even after the most favourable cure, by skin only, or more frequently a thin cicatrix, and the patient is constantly exposed either to actual suffering, or to the dread of it, from rheumatic pains, ulcerations, and other accidents, which are apt to be induced by variations of temperature or the slightest injuries.

Am I not justified then in saying, that this operation is, in every point of view, bad?

How, in despite of the obvious and unanswerable objections, which I have stated, it should still meet with such general patronage, I am at a loss to explain, unless, indeed, the equality which it establishes, as far as amputation is concerned, among all operators good and bad, be its redeeming virtue. If so, I trust that this plan will ere long follow the fate of others resting on the same bad foundation; and, like the gorget, be banished from enlightened surgical practice. *

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* Were I to offer any suggestion for the improvement of an operation which I think ought to be entirely abandoned, I would advise, that the muscles should be cut not on a line with the reverted skin, but as low as they are exposed. There would thus be a greater proportion of flesh to the skin, which, from not being drawn back by the muscles, its connexion with them having been destroyed, would still be amply sufficient for covering the stump. We should thus have the same skin and bone, but two or three inches more of muscle. *See* Explanation of Plate.

There are two modifications of the method by circular incision, which may next be noticed; one of these is that adopted by the celebrated Baron Dupuytren of Paris, where I have often seen him perform it with great dexterity, in the Hotel Dieu.

Instead of dissecting back the skin, Dupuytren cuts all the soft parts at once to the bone, which he next removes after retracting the muscles to what he considers a sufficient extent. But here he is not very particular, as on most occasions (of amputation I mean), he makes no attempt to obtain union by the first intention; indeed I have seen him dress the bone all round with dry *charpie*, previously to *approximating* the edges of the stump, for it was seldom possible to bring them into contact. Why M. Dupuytren persists in following this plan, the result of which is not more successful than might be expected, I cannot tell; indeed, it always appeared to me a most anomalous part of his practice, which, in all other respects, I regarded, from the specimens that I had the happiness of witnessing, as worthy of unquestionably the best anatomist, profoundest pathologist, and ablest surgeon of the age we live in.

I think it but justice to M. Dupuytren to remark, that on one occasion I saw him perform an amputation after his own method with complete success, notwithstanding the most adverse circumstances.

The patient was an old fencing-master, not less, to the best of my recollection, than 80 years of age, whose elbow-joint was diseased so much, as to threaten him with speedy destruction. Amputation was performed about the middle of the arm, and as the soft parts were exceedingly relaxed, they proved amply sufficient for covering the bone and coming into mutual contact. The cure, though opposed by acute pectoral complaints, which did not yield until after free and frequent use of the lancet, went on well and terminated most favourably.

Now, in this instance, I believe that, if more skin had been saved, the result would not have been so fortunate, since, owing to its little elasticity and feeble vitality, every chance would have been afforded for collections of matter in the loose and flabby stump, which must soon have worn out the old man's strength. In such particular cases, then, I think the proceeding of Dupuytren most excellent, rational, and well deserving the imitation of surgeons in this and other countries. It is obvious that the cutting part of the operation is performed almost instantaneously, and consequently with comparatively little pain; the sound uniting surface too, must be perfectly smooth and free from haggling and laceration.

The other modification of amputation by circular incision, consists in cutting the flesh, not at once by a single sweep, but by two incisions, one of which divides the superficial muscles, the other those more deeply seated. The alleged advantage of this proceeding is, that it enables the surgeon to obtain a better covering for the bone, than when he follows the common practice; since, owing to the superficial muscles retracting before the deep ones are cut, the former are left longer than the latter, so that a sort of hollow cone is formed, the apex of which is constituted by the bone. But I do think, that though this reasoning may answer very well in theory, it will certainly mislead, if allowed to dictate in practice. For if the common method be defective, as I have endeavoured to show, from removing too much muscular substance, it is very plain that the double incision must be still more objectionable, as it requires a still higher division of the deep-seated muscles. I may observe too, that unless the surgeon is a very dexterous operator, he will be particularly apt to cut the muscles unequally, so that their surface shall be irregular, unseemly, and little disposed for union by the first intention. I may here give the outline of a case related by Richter in his Surgical Observations, as it not only shows the imperfections of the plan just mentioned, but also affords a good example of the inconveniences which attend amputation by the circular incision, even when performed by the most skilful surgeons. " Various means," says Richter, " have been contrived for preventing the projection of the bone, after amputation of a thigh; all of these, however, have been employed without success." As an illustration of this observation, he gives the case of a boy about 14 years of age, healthy in general, but affected with a disease of the tibia, which, in Richter's opinion, rendered amputation of the thigh necessary. An incision was made three fingers breadth above the knee, which divided not only the skin, but also the cellular membrane, down to the muscles; and was repeated while the assistant held back the integuments with his hands, so as to permit their still higher retraction, until the upper edge of the skin was at least three large finger-breadths distant from the under. " With the third circular incision," to let Richter speak for himself, " I cut through the muscular flesh, by the edge of the retracted skin, not so far as the bone, but only to about the middle of its thickness. Then with the crucial compress, I made this external part of the flesh be pulled up as high as possible; and I now, with a fourth cut, divided the remaining muscular flesh down to the bone, at the edge of that which had been drawn up. The whole flesh was now drawn back so strongly with the crucial compress, that the bone was denuded two fingers-breadth at least, and there sawed through." The cure was completed most happily in little more than a fortnight. But what was the result? "I saw the patient eighteen months after, and to my great astonishment found that all the muscles round the bone had retracted in such a manner, that the bone projected a whole hand-breadth, covered only with the skin." Yet Richter says, "an Alansonian amputation can hardly succeed better than this one did; and I am still of opinion, that the practice recommended by Alanson, of endeavouring to make the stump concave, is very difficult, even impossible."

Having now mentioned the disadvantageous circumstances that induce me to consider the circular incision as a bad plan of amputation, I shall, in the next place, proceed to the second object of this paper, which is to recommend what appears to me a preferable method of performing the operation.

The extreme difficulty, or rather impossibility which must ever be experienced in obtaining a good fleshy stump when amputation of the leg is performed by circular incision, especially when the limb is removed at or below its middle, long ago suggested to surgeons the propriety of saving from the posterior and fleshy part, a flap sufficiently large to cover the bones, and meet with the integuments before.

The advantages of this operation were so obvious, notwithstanding the awkward manner in which it seems to have been executed at first (viz. in the earlier part of last century), that several of our most distinguished and more modern surgeons adopted and improved it. Of these, the one who has done most towards its perfection, is Mr Hey of Leeds. That excellent practitioner wisely remarking, that half of the leg was quite sufficient for preserving the use of the knee-joint, a great matter where an artificial leg is to be worn, determined that amputation nearer the ankle was not only objectionable, on account of the redundance of bony and tendinous parts and deficiency of muscle, but altogether useless, as far as the patient's comfort was concerned. He therefore fixed upon the middle of the fibula as the best point for removing the limb; and has given directions for obtaining, by measurements that will answer in every individual, the dimensions of a flap which may suffice for covering the stump, so that the least expert surgeon can have no difficulty in ascertaining the proper extent of his incisions. With Mr Hey's improvements, the flap operation below the knee may be regarded as nearly perfect. Many surgeons, however, still abide by the old process of circular incision, though I confess nothing appears to me more wonderful than their doing so, provided they ever had an opportunity of seeing the distressing condition which is the inevitable fate of all those who have been treated in this way, no matter how favourable their cure may be.

When the truncated extremity belonging to any of these unfortunate individuals is examined, we find a complete analysis of the constituents essential to a good stump. At one part, there is the tibia covered by skin at the best, and more generally a thin cicatrix only; at another, the muscles form a great projecting mass, altogether useless and inconvenient, instead of insuring the patient's comfort by covering the bones. I cannot at this moment pretend to recollect how often I have seen stumps of this sort bring patients to hospital; but feel quite satisfied, from the result of my observation, that no man, especially in the lower walk of life, can possess one of the kind long, without requiring medical assistance.

The great superiority of amputation by the flap over the common plan of operating below the knee, led some surgeons who paid more obedience to the suggestion of correct principles, than the dictates of common rule, to try the same method in other situations. Indeed, what could be more natural for those acquainted with the circumstances which favour and oppose union by the first intention, than to think favourably of an operation which enabled them to bring all the soft parts into mutual contact, without disturbing their natural connexions?

Yet, strange to tell, the flap operation has never yet met with any decided and unqualified approbation; on the contrary, it has always been performed with an apology, and spoken of as a questionable experiment. Even Lisfranc of Paris, who teaches operative surgery after the newest and most liberal fashion, while he shows the method of amputating by the flap, seems rather inclined to favour the circular incision. In this country, the operation is almost unheard of. Mr S. Cooper, whose opinion I know has much weight with many people, thus expresses himself relative to its merits, in his Surgical Dictionary, page 43, third edition. "The flap amputation is certainly more painful than the common method; and, though it has had very able men for its patrons, it is questionable whether it is productive of the smallest advantage." And again, "The flap amputation of the thigh is now quite abandoned by all the best surgeons in this country, and no description of it seems necessary." Notwithstanding this decided tone of censure, I am inclined to think most favourably of amputation by the flap on all occasions; and my opinion is not founded on theory alone, but rests on much observation; for my friend Mr Liston has used no other operation than this for some years past; and his well known success, in my opinion, shows the excellency of the practice. I also have operated after this plan on several occasions with the happiest result.

Being fully satisfied, then, that amputation ought to be, in every case, performed by the flap, I shall now endeavour to explain the method of operating which is found to answer best. No general rule can be given for determining the requisite size of flaps. It is true, that a knowledge of what proportion there is between the osseous and muscular parts will assist so far. Thus, if there is but one bone, and an equal covering of muscle, it will be best to make two lateral flaps; when, again, there are two bones, and a deficiency of muscle on one side, a single large flap will be obtained from that part where a good one is to be got. But as, in forming the flaps, the surgeon must always cut with that degree of obliquity which may leave the skin and muscle sufficiently long to meet easily over the bones, and as it is necessary here to make due allowance for retraction of the different tissues, it is evident that no practice on the subject can ever teach the necessary precision,—a sort of tact which can be required only by frequently seeing the operation properly performed on the living body. And even with all advantages, the perfect performance of this operation must obviously require eyes and hands much more skilful and experienced than what are competent to execute the common plan by circular incision; but the difficulty is not so great as might be imagined, and, such as it is, seems to me rather an advantage, in so far as it may deter presumptuous individuals from attempting what they are qualified to perform neither by talent nor education.

When the flap operation is performed, the soft parts may be cut either from without inwards, or from within outwards. The first of these methods was the one practised at the first introduction of the plan, and is still, I am informed, employed with great dexterity by that skilful surgeon Langenbeck; but the second is, I think, preferable, as it is done more easily, and seems to give less pain to the patient. * In both cases, the ob-

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* This circumstance is, I believe, owing not only to the greater degree of rapidity with which the incisions are performed, but also to the inner surface of the skin being cut first. Every surgeon must have noticed, that patients express much less pain when abscesses or sinuses are opened by cutting outwardly, than when the same thing is done from without inwards. There is even a difference often remarked in the suffering expressed during the introduction of stitches for the interrupted suture, the patient complaining bitterly when the needle is passed through the skin inwardly, and comparatively taking little notice when it is passed in the opposite direction. It is not easy to explain the reason of this circumstance; perhaps it ought to be ascribed to the branches of nerves being divided previously to the injury of their

ject of the surgeon is precisely the same, viz. the formation of proper flaps, by cutting more or less obliquely, according to the thickness of the soft parts. As I have said before, no general directions can be given for determining the extent of the incisions; I shall therefore content myself with describing the manner in which they may be executed most advantageously.

Lisfranc recommends a long very narrow knife, sharp on both edges; but I think the one used by Mr Liston better calculated for the purpose. It is about six inches in length, and five-eighths in breadth, thin and blunt in the back, except for an inch from the point, which is very sharp; the back is straight and so is the belly, except about an inch and a half from the point, which is slightly convex.

The dimensions which I have stated are fully sufficient for the arm and fore-arm, the leg, and all amputations in children; for the thigh of an adult, a greater length will of course be required.

The surgeon grasping the limb with the left hand, at the place where it is to be removed, ascertains the situation of the bone by means of his thumb. He then introduces the knife over the bone at that part where he wishes to apply the saw, and, perpendicularly to it, he passes close by its side, and so on until the point appears directly opposite. * This finishes the first part of the operation, or the transfixion, as it is called; after which, he cuts his way outwards, in a line forming an angle with the bone more or less acute according to circumstances, so as to complete one flap.

He then embraces the remaining undivided parts with his left hand, and, gathering them together, passes the knife on *their* side of the bone, so as to insulate it completely. Removing the restraint of his left hand, he now forms a second flap in the same way as he did the first.

If there is only one bone, he immediately divides it, his assistant holding aside the flaps with his hands as high as it is exposed; if there are two, he separates the interosseous sub-

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sentient extremities. But be this as it may, there can be no doubt as to the fact, which, though I am aware, in general, little if at all taken into consideration, is nevertheless, in my opinion, deserving of attention from every humane surgeon who thinks of his patient's feelings, and endeavours to abridge, as far as may be possible, the agonizing pain of those cruel measures which necessity alone can justify.

* There is some difference of opinion as to whether this first flap should be formed from the external or internal side of the limb. But as this seems to me a matter of perfect indifference, unless in reference to the operator's convenience, I would advise the surgeon, unless he be ambidexter, always to pass the knife, in the first instance, on that side of the bone which corresponds with his right hand as he stands before the patient.

stance, and does the same thing; thus finishing his operation, which, with reasonable haste, and without the smallest hurry, need never occupy more than half a minute at most.

It is impossible to imagine a greater contrast than that afforded by a comparison between the wound which results from this operation, and that caused by the method of circular incisions.

The bone, instead of standing naked and conspicuous, can hardly be discovered until the flaps are laid aside. The muscles, not now deeply retracted, and exposing an abrupt, ragged termination, extend far beyond the bone, and display two smooth equal surfaces, amply sufficient for coming into mutual contact, and well disposed for reciprocal union. Lastly, the skin, so far from forming a loose and hanging border about the wound, is left in undisturbed connexion with the subjacent parts, and in proportion just sufficient for supporting and covering them when the two flaps are brought together.

The difference of appearance is not less remarkable, after the wounds have been dressed.

In the one case, the line of union is straight, in the other it represents the arc of a circle;—in the first there is puckering of the integuments, in the second there is none;—in the former, the muscles form projecting and inconvenient corners; in the latter, they exist only when they are most required, *i. e.* over the bone.

Union by the first intention most commonly attends amputation by flap; but, if suppuration does become established, there is much less inconvenience than generally happens. And I may here remark, that many as the operations, which I have seen of this kind, are, I never yet met with an instance of the bone protruding or exfoliating. I may notice, too, that the inconvenience mentioned by Richter as the greatest attending the amputation of the thigh, viz. the extensive and long continued retraction of the posterior muscles, is avoided, as those only on the lateral aspects are employed to cover the bone.

The difference of comfort enjoyed by the patient after the cure, is also most decidedly in favour of flap method, since the stumps formed by it, instead of being conical and skinny, are uniformly round and fleshy.

Having said so much relative to amputations in general, I may now make some observations on those in which the bone is not divided, but is removed from its articulating cavity.

Until of late years, amputation by disarticulation was an operation hardly tolerated by surgeons, who, from some old and deep-rooted prejudices concerning cartilaginous surfaces and synovial apparatuses, regarded the articulating extremity of a bone as an almost insuperable obstacle to healthy and speedy union.

Now, however, we know, that the synovia is not secreted by a *glandular* apparatus, since none exists *really* in any joint, and not even *apparently* in many where that fluid is found in abundance, but is the product of a serous bag, which ceases to pour forth its lubricating product as soon as all occasion for its presence is taken away, by the removal of the bone, whose friction it was intended to lessen.

We also know, that cartilage, though possessed of feeble powers as far as the process of granulation is concerned, nevertheless does not act as a foreign body, in keeping up irritation and preventing union, since perfect and speedy cures frequently follow amputation, even when performed at the largest joints of the body. Why, then, surgeons should still scruple as to performing the most trifling disarticulations, and persist in using their retractors and saws for the partial and total removal of fingers and toes, thus making tedious operations out of what might be done much better, and almost instantaneously, by disarticulation, it is not easy to explain; unless we suppose the power of habit is as powerful here, as we know it to be on some other occasions.

Mr Lisfranc is a great advocate in favour of disarticulation, and teaches the method of operating for most of the joints of the extremities,—indeed all of them, with the exception of the ankle and knee. He does not, however, approve much of amputation at the wrist and elbow. It is at the joints of the fingers and toes, those of the first and fifth metacarpal and metatarsal bones, that of the metatarsus, with the tarsus, together with those of the shoulder and hip, that he decidedly recommends disarticulation.

The plan of operation in all these cases is, generally speaking, the same, viz. to cut into the joint on one side, and out of it at the other; taking care that, in doing so, one or more flaps are preserved from the parts best able to afford them. The instrument employed is on all occasions a long narrow knife, the dimensions of which will of course be varied according to the size of parts concerned. For the fingers and toes, a common straight operating bistoury is sufficient; for all other cases, a much larger knife is required; and for the hip-joint, one not less than a foot in the blade is necessary.

I shall now describe what seems to me the best of Lisfranc's operations for disarticulation,—not with his exactness and extreme minuteness, which, I am inclined to think, tend rather to confuse than instruct, but in such a manner as may enable any surgeon, properly qualified in other respects, to carry them into execution when he thinks proper to do so.

In amputating the first and second joints of the fingers and toes, a single flap, sufficient for covering the bone, is saved from the palmar aspect. The operation may be done either when the hand is held in pronation, or placed in supination. In the first, the surgeon cuts right into the joint with his bistoury, which he then carries closely along the surface of the phalanx about to be removed, until he obtains flap enough for the purpose. In the second case, he pushes his knife through the finger parallel to the bone, and close by the joint. He then detaches precisely the same flap, which, being held up by the assistant, he finishes the operation by cutting directly through the joint.

In separating the fingers and toes at their metacarpal and metatarsal joints, two lateral flaps are formed, one in entering the knife into the articulation, the other in cutting its way out.[*]

Passing over the removal of the first and fifth metacarpal bones, and that of the metatarsus in whole or in part, I shall proceed to describe two of Lisfranc's operations, which, of all those he has contrived, deserve the highest approbation—I mean those for disarticulating at the shoulder and hip-joints. In amputating at the shoulder-joint, he introduces the point of the long knife usually employed at the hollow, between the anterior edge of the clavicle and coracoid and acromion processes; he pushes it backwards and downwards, so as to pass through the capsular ligament, and brings it out at the posterior and lower margin of the axilla. He then cuts outwards, following closely the direction of the head and neck of the humerus, until he obtains a flap from the deltoid and other muscles, sufficiently large. This finishes half of the operation, which is completed by passing the knife round the bone, and cutting away a second flap from below. No operation can be more speedy, simple, and better calculated for obtaining good flaps than this, which Lisfranc actually performs on the dead subject in two seconds.

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[*] Candour is a never-failing ingredient in the character of a good surgeon.

Mr Liston was in the habit of amputating fingers and toes, by making two lateral flaps with a scalpel, and dividing the bone by means of the cutting pliers, which he has contrived and employed so extensively and successfully in operating on diseased bones; but on learning from me the method of Lisfranc, described above, was so much pleased with its facility and other advantages, as to adopt it almost exclusively.

In the same way, the illustrious Baron Dupuytren made use of the instrument just mentioned, at my suggestion, with the most perfect success, in the Amphitheatre of the Hotel Dieu, and in a case, too, where even he expected to meet with very considerable difficulty.

Mr Liston, however, still as formerly, employs the cutting pliers when it is necessary to take away a portion of the metatarsal or metacarpal bones; and no method can be better calculated for the purpose, since it is easy, certain and speedy, enabling the surgeon to perform instantly, what, with the employment of half-headed trephines, chain-saws, Machel's saws, circular saws, and other ingenious toys, would, if perfected at all, require hours of hard work.

In operating for the hip-joint, Lisfranc proceeds in the following manner:—The patient being placed on his back, he introduces the point of a very long double-edged knife into the fore-part of the thigh, over, or rather a little above, the articulation of the femur with the acetabulum. * He pushes it close past the neck of the bone, and brings it out on the opposite side of the limb, a little below the tuberosity of the ischium. Then he cuts outwards, following the shape of the trochanter major, and proceeds close down along the femur for an inch or two below that process. He at last turns the edge of his knife, and completes the first or external flap. The arteries which have been cut should now, according to his advice, be tied. The second part of the operation consists in forming an internal flap, by grasping the undivided parts in the left hand, and passing the knife close to the inner side of the neck of the femur, so as to detach it from all connections with the muscles and then cutting downwards, attending to the shape of the trochanter minor, below which the knife should be carried for an inch or two, and then turned gently outwards, so as to finish the second flap. It is obvious to one acquainted with the anatomy of the parts, that, as long as the surgeon merely cuts downwards, and keeps close by the bone, he will not injure the femoral artery, which cannot be divided until the knife is carried outwards: And here lies one great excellence attributed by Lisfranc to his operation; for before the surgeon cuts the femoral, the assistant is enabled to introduce his fingers at the solution of continuity, and compress the vessels. The arteries of this flap also having been secured, nothing remains but the

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* To determine the proper part for this purpose, Lisfranc advises us to measure three quarters of an inch directly inwards from a point an inch and a half distant from the superior anterior spinous process of the ileum, in the direction of the knee; but I very much distrust the propriety of giving any such directions; for I have invariably seen *measurements* mislead, when trusted to in performing an operation; and even granting, for the sake of argument, that they may indicate the position of parts concerned in some operations, still I think it will not be denied that they are altogether useless to an anatomist; and it is to be hoped, that no man who is not an anatomist will attempt any important operation.

third part of the operation, or the disarticulation, properly speaking.[*]

To effect this, the surgeon, seizing the limb with his left hand, while the assistant holds aside the flaps, makes a cut half round the margin of the acetabulum at its fore part, with the same keenness as if, says Lisfranc, he expected to cut through the head of the bone. The capsular ligament having been thus fairly opened, he performs abduction, starts the bone from its socket, passes the knife round its head, cuts the triangular, and what remains of the capsular ligament, and so finishes the operation.

Explanation of the Plate.

Fig. I. illustrates the imperfection of the common method of amputating by circular incision. The different lines denote the height at which the skin, muscles and bone, are cut.

Fig. II. shows the plan of Baron Dupuytren. The muscles retract, and pull the skin along with them; so that, after the wound is dressed, a gap is left.

Fig. III. represents the advantages of my proposed improvement on the circular plan of amputation. The muscles are left in sufficient length; and the skin, not being drawn back by them, is competent to cover the stump.

Fig. IV. is meant to explain the great superiority of the flap operation over every other. The black line marks the direction of incisions—the dotted one defines the extent of flaps after retraction of the muscles.

Fig. V. is a plan of Lisfranc's method of amputating at the hip-joint.

V.

Observations on Amputation.[†] By Robert Liston. Fellow of the Royal College of Surgeons, London and Edinburgh, Lecturer on Surgery, &c. &c.

Though the operation of amputating limbs has been much improved in this country within the last sixty or seventy

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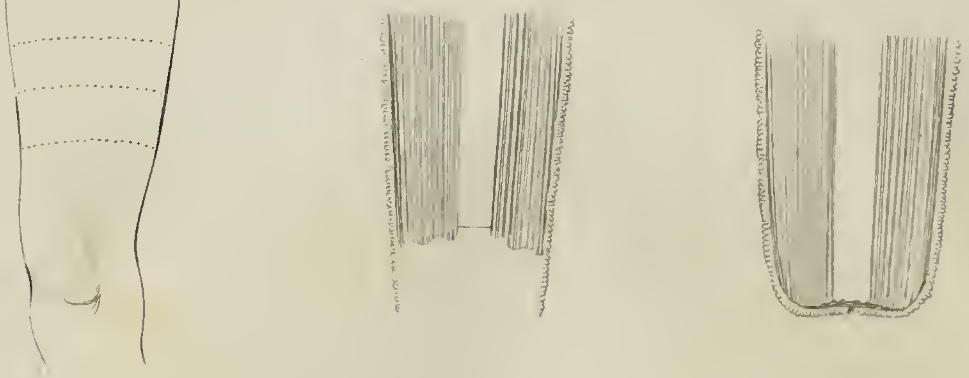
[*] Had I again to amputate at the hip-joint, I should still defer securing the vessels until the disarticulation was completed.

[†] These Observations were sent to us as part of Mr Liston's paper on Caries, but, with Mr Liston's permission, we have printed them separately as a sequel to Mr Syme's paper, with which they appear to us to be more intimately connected.—*Editor.*

I



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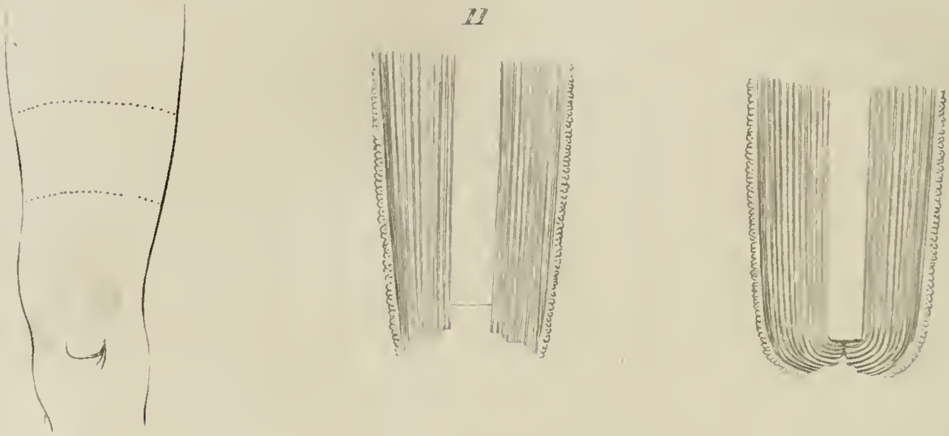
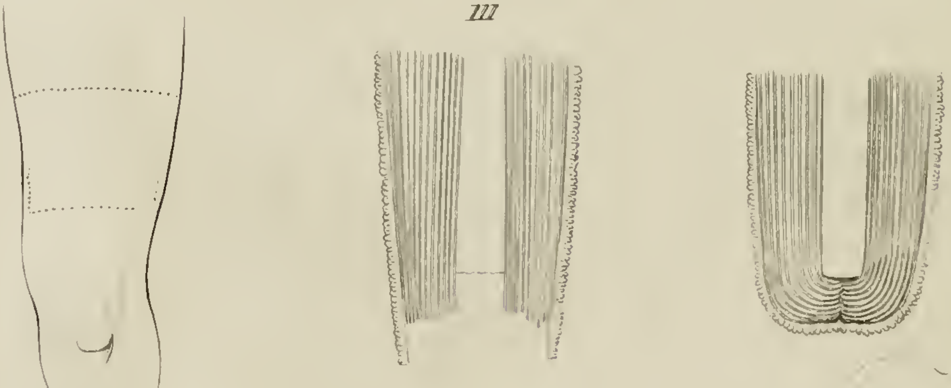
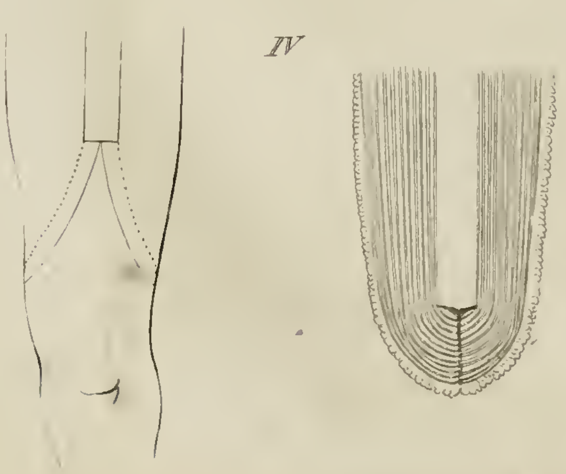


III



IV



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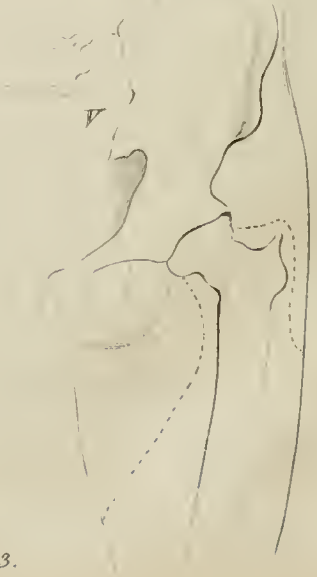

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years by O'Halloran, Alanson, Hey, &c. still it does not appear to have as yet attained to the utmost point of perfection. To me it appears, that it would be well to reject several manœuvres now practised, whilst some neglected proceedings might be with advantage revived. Were the modes of operation, which I am in the habit of putting in practice, generally adopted, the cases of amputating instruments might be emptied of their contents, with the exception only of the saw, which, by the way, is seldomer required than might be supposed.

In the first place, then, as to the tourniquet, without previously applying which, few practitioners will make the slightest scratch on the extremities; it is, in my opinion, of no use, and, in many cases, it is worse than useless. If the surgeon does not think himself (or rather his patient) safe, without the compression of a pad on the artery, with a circular strap betwixt the centre of the circulation and the part on which he is to operate, a much simpler apparatus than the screw-tourniquet may be had recourse to—a silk handkerchief and a bit of wood. But any means of this kind, tourniquet or other, whilst it arrests but imperfectly the flow of blood in the arteries, produces great engorgement of the veins. Unless the tourniquet is screwed with extreme tightness, or, supposing that it has been so screwed, it is at all relaxed, so as to allow the surgeon to find the mouths of the vessels, bleeding to a considerable extent always occurs—say to the extent of a pound, or even more. Many patients are so exhausted before they can be induced to submit to the operation of amputation, as to bear this loss with difficulty. The pad of the tourniquet cannot be applied so high in the thigh, owing to its form, as to command the deep-seated arteries. The blood is poured freely through them; and finding its way also into the veins, is evacuated from their cut extremities also, especially from the more superficial ones, in which the return is most effectually retarded by the circular band. In fact, such a degree of general pressure on a limb, as will have but very little effect on the arterial circulation, will most effectually arrest the flow of blood in the veins, their coats being thinner and more compressible, and their circulation much more slow and languid; whilst, if the operation is peformed by circular incision, the retraction of the parts will be in a great measure prevented. The hemorrhage even from the femoral vein is often so profuse when the tourniquet is employed, as to have induced that late excellent surgeon, Mr Hey of Leeds, to recommend, and many others to practise, the dangerous expedient of including it in a ligature. When we see, in an ordinary amputation of the thigh, eight, ten, or a dozen of vessels tied, the tourniquet in the meantime being screwed and unscrewed; and when we know that, in similar circumstances, when no such contrivance is had recourse to, the hemorrhage is equally well arrested by two or three ligatures, little doubt can remain, but that the greater number of bleeding orifices have been veinous. The bad effect of tyeing a single vein, even under favourable circumstances, has only to be hinted at, in order to show the impropriety and danger of this practice. When a surgeon wishes to distend the veins of an extremity, he does so by surrounding it by a tight circular band, and this is not the state most favourable for making any extensive incisions into it.

The adjusting of the tourniquet is productive of delay, and always harassing and painful to the patient's feelings.

The greatest of all amputations, those at or near the shoulder, or hip-joints, are readily accomplished, without any such preparation; and why not those lower down, where fewer and smaller vessels are divided, and better opportunities afforded of applying pressure to the principal vessels? Notwithstanding what Mr John Bell (than whom no better authority on many surgical points can be adduced) has so eloquently urged pressure complete enough, not only to stop the pulsation of an artery in a limb, but also to arrest completely the flow of blood, can be easily applied by the fingers only.

So much have I been impressed with this fact, (and I only mention the circumstance to show that my impression was correct), that I have repeatedly, when no proper assistant was at hand, compressed both the femoral and humeral arteries with the fingers of one hand, whilst with the others I removed the limb, and with the loss of much less blood than if I had followed the ordinary mode. This, however, is an experiment there is no necessity for repeating. The assistant-surgeon can apply the pressure whilst the incisions are made. One of his hands are free afterwards to take hold of the vessels with the forceps. The pressure does not require to be very great, provided it is properly applied. The subclavian, for instance, can be easily commanded against the first rib, and the femoral upon the brim of the pelvis, so as to arrest the flow of blood into the limb.

In amputations lower down, it may be convenient to make the pressure lower; and there are many points at which it can be so made. Opportunity is afforded of doing so completely and freely; for, with the forceps in the one hand, and compressing the principal artery with the other, if the assistant does not readily see the mouth of a vessel, he can, with the greatest facility, relax his grasp a little, and again instantly make compression as occasion may require. There is here no twisting or untwisting, or screwing or undoing the pressure; it is all accomplished without confusion and bustle, or even the appearance of it. From a good deal of experience, I can vouch for the perfect safety of this plan.

The mode of amputating by the circular incision is liable to many weighty objections, especially as it is ordinarily practised; the first, and a great one, is, that it is complicated, and requires a considerable space of time for its performance.

When the time necessarily demanded is lengthened out through the unsteadiness or incapacity of the surgeon, the suffering and danger to the patient is excessive, so much so indeed, that many sink before the first dressing. When the skin is dissected from the fascia, and turned back, like the sleeve of one's coat, (to say nothing of the cruel delay it occasions), being deprived of its vessels, it generally sloughs; and as is often the case if the bone has no other covering,—forth *it* comes also, part of it in its turn dies; and if the patient has still strength of constitution left to bear up against the discharge and suffering, it also is thrown off.

A stump thus formed can never have any of the qualities of a good one. I do not mean to deny that a good stump can be formed by the circular incision. By retracting the integuments, and with the point of the amputating knife † dividing the slips of cellular substance which connect the skin to the fascia, and then cutting the muscles, turning the edge a little obliquely upwards; or, perhaps, by using the knife in such a way as to make in its course round the limb two flaps on the inner and outer side through both integuments and muscles, a very good fleshy stump may be obtained; but still the time requisite for all these various manœuvres is greater by ten times than in the plan I propose recommending, and the operation certainly altogether more tedious than there is any occasion for.

Neither the retractors of tin, leather, or cloth, the tenaculum nor the bone-forceps, are, or should be at all necessary; and unless the surgeon or assistant who supports the limb is very inattentive, the latter can never be required.

In the flap operation, the holding one of them aside allows the saw to be applied when that instrument is required. With the tenaculum it is a very difficult matter to take hold of the artery, so as to pull it out quite clean. The dissecting forceps can be used for that purpose with better effect, whilst that instrument is also in many operations of great use otherwise.

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† The changing of instruments adds greatly to the length of time spent in this and other operations—such as the order I have seen them used in: Tourniquet, amputating knife, scalpel, band to retract the folded integuments, amputating knife, catline, retractors, saw, bone-nippers, tenaculum.

For a considerable time past I have had recourse to flap operations, from witnessing the bad effects of other modes (I am ready to allow not always very well executed), and from a desire of having a good covering of both skin and muscle for the end of the bone. I have every reason, in the successful and speedy termination following such operations, to continue in the same course. By cutting from within outwards, the skin is left adhering by its own vessels, &c. to the subjacent parts, and, without trouble or loss of time, a good stump is at once formed. The operation is applicable to every part of the extremities, whether the bones are to be disarticulated or cut across.

In the thigh or arm, the flaps are best made by grasping the parts on one side, and pushing a long, sharp-pointed knife through them close by the side of the bone. The longer the knife, the more speedily and easily, both for patient and surgeon, will the flap be formed. In fact, it may be completed frequently in pushing the knife through, without retracting it at all, or having recourse to the sawing motion. Immediately on making the one flap, the knife is passed on the other side of the bone, so as to come through the former incision, and then brought out after the same fashion.

A single turn with the point of the instrument will be sufficient to separate what of muscular substance remains close to the bone; and, by holding aside one of the flaps, the saw can be thus readily applied, and the amputation finished. But a few seconds will suffice for its performance—in most instances not above ten or twelve.

VI.

Essay on Caries, and its Treatment. By Robert Liston, Fellow of the Royal Colleges of Surgeons, London and Edinburgh, Lecturer on Surgery, &c. &c.

Amongst the many brilliant improvements which have been effected in this country in the cure of surgical affections, some diseases appear to have been comparatively overlooked and neglected. The disease known under the term *Caries* is one of these; and the active practice of our forefathers (by them employed indiscriminately enough), has not only been abandoned for one most inert, hurtful and unsatisfactory, but the symptoms and appearances, of themselves sufficiently distinctive, have been confounded in many works with those of diseases most widely different in every respect. As this Essay may probably be followed, at some future period, by notices of other diseases and injuries of the bones, it may be proper to premise a few remarks on the pathology of this part of the structure.

It is well known that bones grow and are nourished by the same means as other parts of the system, and are subject to the same laws. Together with the other white textures, they are less freely supplied with blood-vessels and nerves in proportion to the other parts, more especially after their growth is completed.

When incited action of the blood-vessels in their harder structures takes place, their sensibility is roused to an exquisite degree, and their healthy or perverted processes are carried on with great vigour and amazing rapidity. Solution of continuity in bones is repaired by the agglutinative process, or union by the first intention; or if, from the nature and circumstances of the injury, or the improper interference of art, Nature is in this thwarted, the breach will be repaired by granulation. The granulations will be found to shoot from the broken surfaces, the vessels of which become extremely active, and the callus or new bone is the result of their assimilation or consolidation. To the surrounding soft parts has been attributed a great share of the work in the union of broken bones; and when bones have been fractured in circumstances not admitting of this assistance, the process of reparation, it is said, cannot be accomplished.

In dissecting a fractured limb which has been removed during the process of union by callus, it will be found that the new bone is uniformly attached to the sound part of the old, the vessels of the part employed in this process being much increased in size. The newly deposited bone, which in its turn carries on the process, being perforated by numerous and large foramina, for the entrance of corresponding ramifications of arteries, &c. The new formation will be perceived shooting from the opposed ends till these are united; and the masses in which they are deposited will be direct, and but slightly prominent, or, on the other hand, irregular and unshapely, according as the separated ends are favourably or unfavourably placed. I can conceive it possible, and in fact have frequently found, new bone connected with the soft parts; but this was the product of a splinter, which had still retained its vitality, and *whose vessels* had formed a contribution to the general action.

Great powers not only in the production, but also in the removal of bone, have been long allowed to the periosteum. No one, I will venture to say, has as yet detected this membrane in either of these acts. New bone has not been found adhering to the periosteum, either in fractures or necrosis, far less has a complete substitute, composed of the *ossified periosteum,* been ever discovered incasing a sequestrum. In every instance, the new formation is deposited in nodules adhering firmly to the old bone, and, as remarked above, freely perforated by nutritious arteries. The vessels of the bone no doubt are ramified on the external and internal periosteum; but it is only after their entrance into the perforations, that they become disposed to pour out ossific matter. In the healthy state of the system, the vessels of each part repair any breach by a substance nearly resembling itself. Skin is united to skin, muscle to muscle, and by substances somewhat similar. We might, therefore, with equal propriety and truth attribute the reparation of skin to the subjacent muscle, or, *vice versa,* as of bone to periosteum, or periosteum to bone.

From various causes, bones are liable to attacks of inflammation; and the terminations are, as in the soft and more sensible parts, in resolution, suppuration, or death. Effusion, by which the vessels so readily relieve themselves in softer textures, cannot take place here so easily, or to such an extent, as to prove beneficial; and it is with difficulty that such deep-seated actions can be controlled by the common remedies. The consequence is, that resolution is comparatively rare; whilst suppuration on the surface, or in the centre of the bones, and death, partial or total, are the more frequent results of injury or incited vascular action, however induced. Suppuration in the substance of bones is necessarily attended with loss of substance; collections of matter, *external* to bones, if allowed to press long on their surface, or if firmly bound down by strong and unyielding sheaths, will, after a longer or shorter time, produce a breach of continuity in them, by causing absorption of their outer lamella, and the subjacent cancellated texture, the pressure of aneurismal or other tumours, have the same effect. Such losses of substance are, in general, speedily repaired, as I have stated above, so soon as the cause is removed, by the active effusion of new matter from the proper vessels of the bone. The impression, that part of a bone has been absorbed by the pressure of an aneurism, ought not to induce the surgeon to prefer the amputation of the limb to the ligature of the artery. *

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* By the ligature of the femoral, which appears to me the more advisable as well as the less hazardous proceeding of the two in such circumstances, I have been so fortunate as to bring to a happy termination a case of popliteal aneurism, where there was every reason to believe that absorption of the bone, to a great extent,

The bones composing the larger joints, even after great loss of substance, are frequently found firmly united by new bony matter; in other words, anchylosed—for instance, the femur and os innominatum, after the removal of the head and neck of the femur, and destruction of the cotyloid cavity, or the knee or elbow-joints, after long-continued disease. Cases, however, are by no means uncommon, in which the loss of substance is of a most intractable nature, and frequently incurable.

Such affections of the bones, resembling specific incurable ulcers in the soft parts, may proceed either from the debility of the patient's constitution, or from the diminished vitality of the surrounding and affected parts, occasioned by long-continued or violent action. Whatever the cause may be, whether constitutional or not, certain it is, that some ulcerations in bones heal kindly enough, whilst others, apparently in the same circumstances, have no such disposition. To such ulcers in bones as resist the natural efforts of the constitution towards their cure, and require the active interference of art, I would propose to limit the term CARIES.

Before taking notice of the circumstances and symptoms attendant on this affection, it may be remarked, that the comparative frequency of the one or other of the terminations of inflammation, suppuration and its consequences, or death, depends much on the kind of bone implicated. The flat bones, the shafts of the long bones, and the outer lamella of all bones, as possessing a more dense texture, containing a greater proportion of earthy matter, and being less freely supplied with blood-vessels and nerves, possess less power of resistance; and necrosis, or death of the affected part, is the common consequence of injury or of vascular action, increased beyond a certain point. Of this disease hereafter. That on which it is now my intention to make some remarks, takes place almost uniformly in the heads of long bones, or in the similar texture of short bones. They have greater powers of life; and their structure, being loose and spongy, effusion and abscess easily take place in them; or otherwise, their outer lamella, being very thin, readily yields to the pressure of matter from without, and admits it into their internal cells.

Though caries is the disease to which the soft and spongy bones are most liable, it does not follow that other diseases may not now and then occur in them. Affections sufficiently violent, and injuries sufficiently severe to occasion death, will sometimes

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had taken place, and exceeding, by several inches, one I had seen amputated. The tumour and limb in the one I allude to, in which the femoral artery was tied, measured nearly two feet in circumference.

happen to such textures. I have witnessed many examples in point; and the preparations illustrative of this fact are pretty numerous in our collection. One very remarkable is from a young man, Cameron, who was struck by a musket ball on the head of the tibia, at one of the engagements in the Peninsula. The wound never completely healed, though at some times the discharge almost dried up, and his health recruited a little. He applied to a public hospital for relief, and was there under treatment for a long time; that is to say, he was kept on low diet, and the wound poulticed. He had an attack of erysipelas, from which his worn-out constitution did not recover. On examination after death, a cloaca was found leading to a sequestrum in the head of the bone, of about an inch long, and half an inch thick, beautifully macerated in the purulent discharge. It resembles a piece of loaf-sugar which has been dipped in hot water, and then dried. Considerable effusion of new bony matter had taken place on the fore-part of the bone, over the cavity containing the dead part, showing how willing Nature was to supply the deficiency. The two diseases of Caries and Necrosis are frequently complicated with one another in the same part; that is to say, the parietes surrounding a sequestrum may become indisposed to heal; and though the dead part be cast out by Nature, or removed by the surgeon, still a cure cannot be obtained without the active interference of art.

The case of a boy, Kennedy, æt. 15, illustrates this proposition in a remarkable manner. He was, without any assignable cause, attacked by violent pain in the upper part of the tibia, about four years before his application to me. An abscess formed and discharged. He was admitted into the Royal Infirmary, when a sequestrum was removed. At the end of five weeks, he was dismissed, and again taken in for eight days, on account of the continuance of the discharge. On his application to me, it was found that the probe led to a considerable cavity in the bone. The opening was enlarged by the application of a large trephine; and after the removal of the diseased cancellated texture, by the scoop and gouge, the cautery was freely employed. In a few weeks, a firm and permanent cicatrization was brought about, and the boy remains free of disease.

It was formerly remarked, that where the action of the vessels is incited in bones, they become highly sensible. Hence, in the commencement of this disease, which is attended with more or less inflammatory action, the patient generally suffers most excruciating agony—so great in general as to prevent him, perhaps for weeks, from enjoying the least repose. The affected part is generally swollen considerably; but the enlargement is seldom so general or great as in the diseased state of the ligaments and other apparatus of a joint, generally known by the name of White Swelling. White swelling, however, may now and then be the precursor of caries; or, in other words, a disease commencing in the ligaments, bursæ, &c., occasioning great enlargement, may extend to the bone; and breach of continuity, terminating in unhealthy ulceration, may ultimately be the consequence. But when the bone is primarily affected, there is seldom much thickening of the surrounding parts; and the swelling occasioned by the effusion of new bone is, as might be anticipated, much more unyielding than that arising from thickened tendons, ligaments, &c.

In the disease in question, the affected portion of bone would appear to be, at first, in a half dead state, neither possessing vitality enough to enable it, by an effort, to repair the breach, nor dead enough to be thrown off by the surrounding sound parts. When the parietes of the breach have remained a considerable time in this inactive state, the surrounding vessels become increased in activity, and the surface of the bone in the vicinity is studded with nodules of new bony matter. This is not in general limited to the affected bone, if one only is the seat of the disease, but frequently extends to those articulated with it. The soft parts are commonly more or less thickened, and rendered exceedingly dense, by the effusion of lymph into the cellular texture; and so great is the thickening in some cases, that the knife is resisted as by cartilage. Such a state of the parts, renders the securing of vessels, after operation upon them, a most difficult task. This I have experienced in several cases. When the bleeding is from small vessels, pressure is here more effectual perhaps, than in the natural state of the parts; but bleeding from a large vessel is most obstinate and troublesome,—the vessel cannot retract, nor indeed can any of the immediate natural processes for the stoppage of hemorrhage take place. It is equally impossible for the surgeon to draw an artery out and tie it in such a state of parts; the only remaining plan of procedure is by the needle and ligature.

I was under the necessity of securing the anterior tibial in the case of the patient B——,* some days after the operation, in this way; and after the removal of the metacarpal bone of the great toe from a man of the name of Cross, great difficulty was experienced in arresting the hemorrhage;—the division of the bone was made with the cutting forceps. To expose the bleeding vessel completely, it became necessary, after all, to turn out the articulating head of the bone. Still it was impossible to pull out the bleeding extremity. The needle and ligature were had recourse to. Notwithstanding this, secondary bleeding to an alarming extent occurred, and was at last commanded by accurately applied pressure.

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* This case is related fully in the subsequent part of the paper.

The discharge which proceeds from such a disease, and which is found to be poured through one or more openings, is frequently of the most fetid kind, and in great profusion—the skin is excoriated, and the probe blackened by its acrimonious and fetid properties. The surrounding bones being deprived so far of their nourishment, become so soft as to be easily perforated by a blunt instrument, or cut by a knife—and, possessing little earthy matter, when separated by maceration from the surrounding parts, they become exceedingly light.

The softening of the bones is frequently not limited to those in the vicinity, but pervades more or less the whole system, and indeed exists in many instances, independently of any active disease. This state depends on the nature of the constitution, and must be attended to accordingly. Carious ulceration is generally confined to one or two bones, at other times it involves a whole chain of them. It may be limited to a part of one bone only in a complicated joint, or may embrace the whole of it. This will depend on the severity and extent of the primary action, or on the length of time and violence with which pressure has been allowed to exist, whether from the nature of the superincumbent texture, or the carelessness (or whatever it may be called) of the surgeon. *

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* The propriety of evacuating matter, when collected upon bones, in their vicinity, or in their substance, cannot be too much insisted upon. Many such collections arise from a primary affection of the bone, whilst others, commencing in the soft parts, will frequently find their way more deeply, owing to the unyielding nature of the superincumbent textures. In the first case, if left to itself, the disease slight, and perhaps limited to a very small portion of one bone, may destroy the whole of that, and attack those in its vicinity. Again, a collection not originally arising from disease in a bone, will now and then deprive a bone of its periosteum, and induce necrosis or exfoliation (degrees of the same affection), or may give rise to all the mischief in a joint as above described.

Equally, if not more dreadful effects arise from collections in the softer textures. Important cavities or canals are also apt to suffer from neglected abscesses in their vicinity. Some practitioners, however, are apt to look upon indolent tumours and abscesses as quite harmless, and advise that they be left to Nature.

In confirmation of what is stated above, I may merely allude to the case of a young girl who died from injury of the head. The existence of matter under the cranium, was clearly enough indicated by the general and local symptoms. The operation was however delayed. On dissection, the abscess was found to have made its way into the longitudinal sinus, and had filled the whole venous canals within the head.

The destruction of the constitution attendant on this disease is at first very great,—the sympathetic fever is followed by a no less violent state of hectic, under which and the other attendant discharge, many patients sink. The general affection, of course, in some degree keeps pace with the local one in violence and duration. The irritation is in some cases so violent as to destroy the patient in a very few weeks or months; but cases will sometimes present themselves, in which a constitution by no means strong, will be enabled to bear up for a long period of years under pretty extensive disease of a bone. By this it is not to be understood, that, during all this period, the patients suffer from hectic. The disease comes into a quiet state, and the exhausted constitution is allowed time to recruit, so as to meet any new attack of irritation and fever.

Such, in general, is the course of the disease; the giving way of the skin is attended with discharge more or less profuse, and with hectic fever more or less violent, according to circumstances. These, as noticed formerly, either destroy the patient, or after a time subside. The discharge will sometimes appear almost entirely to dry up, and some of the openings may even close, allowing the constitution to rally a little. The temporary cicatrices of sores leading to diseased bone, have a very different appearance indeed from the sound scar after their removal. The former is bluish, soft, and elevated above, and on a level with the neighbouring parts, and moveable over the bone; the latter are depressed, white, and firmly adherent to the bone.

The paroxysms of pain and inflammation in the diseased part occasion fresh attacks of constitutional derangement. This goes on till the patient's health and strength are exhausted, and he is then either destroyed entirely, or is relieved by the spontaneous or artificial removal of the offending part. A natural cure can take place only by the complete death of the affected part, and its consequent separation from the system.* This

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I may also notice instances of matter confined in the neck, making its way into the mediastinum, trachea and œsophagus, several of which I have witnessed. The occurrence of disease in bones from this cause, must be sufficiently familiar to every one who has enjoyed even limited opportunities of observation.

* A natural cure of caries, though very rare, is certainly not absolutely impossible. A bone, or part of a bone, affected by this disease, and to which alone the mischief is confined, may, from some circumstance, become necrosed and separated from the system. This occurrence, however, is so extremely rare, that the surgeon is not justified in encouraging the patient to hope it. The disease, when fairly established, will endure for a series of years, at one time becoming very active, and again subsiding into a quiet state. The patient's health, notwithstanding, gradually declines, and, in many instances, so much so as to preclude any attempt at saving the extremity in which the disease may be seated.

may arise from an attack of inflammation greater than the worn-out parts are able to withstand; and if the whole disease is thus destroyed, it is immediately thrown off, and granulations speedily springing up from the surface repair the breach. We frequently find small pieces of bone lying loose in a carious cavity, and such are now and then discharged during the progress of the disease. These are in general separated by the rapid progress of ulceration; but the ejection of many such is not sufficient to make any favourable change in the disease. The whole unhealthy part *must become necrosed,* and be thrown out before the discharge, and consequent constitutional irritation, can be expected to subside. As Nature does so little in this disease, we have now to consider what prospect of relief our art holds out.

From what has been said above, it would appear, that to trust to Nature for a cure in this diseased process, is utterly fruitless and unprofitable; nor can the application of external remedies, as ointments, poultices, &c. if they do no harm, be reasonably expected to do much good. We must therefore adopt some means of getting quit of the mischief, and that before it has undermined the constitution, and ruined the patient's health. The first, or inflammatory stage of the disease, and which, as has been remarked, is attended with the most excruciating pain, must be counteracted by the local abstraction of blood (though this remedy cannot with safety be carried to a great extent), followed by counter-irritants, issues, sinapisms, blisters, or ointment of the tartrate of antimony, used so as to bring out a copious eruption of pustules. These three last I would prefer in mild cases. From the employment of all of them, I have witnessed the most happy effects. The tartrate of antimony is perhaps to be preferred, as being quite manageable, so that as much or as little irritation as is desired may be produced without difficulty, and in a short time. I am in the habit of ordering it daily, and observing its operation both in affections of joints, and in fixed deep-seated pains, with the most marked and decided success. The most effectual remedy, however, and the one from which most relief is to be derived, both immediately and permanently, is the application of the actual cautery, in the form of moxa, to the integuments over the diseased part, for the purpose of producing a pretty deep eschar. In all deep-seated pains of the joints, this remedy affords the most speedy and complete relief, at the expense of but a trifling pain of no long duration. The pain does not appear to be greater than that arising from the formation of an eschar by potass, or any other of the potential cauteries, and lasts only during the time of the application, whilst the violent pain does not subside perhaps for twelve hours after the employment of the potass. The pain of the moxa, I repeat, is only instantaneous; its beneficial effects are both speedy and lasting; the formation of an eschar by the potential cautery produces severe pain of long duration, and its use generally leads to disappointment.

In a disease of some standing, or in a large joint, the application of one moxa will prove insufficient; and it may become necessary to apply several around the pained part. If it is desirable that the eschar should be deep, they must be repeated in the same place. The good effect is generally experienced before the separation of the eschar; and, upon its falling off, the healing of the sore should be encouraged. I never could observe the slightest benefit from maintaining a discharge from an issue. However made, the patient feels no relief after the discharge is established; and, if profuse, it must be a mean of destroying his health. Instead of encouraging discharge, it will be found more advantageous to have recourse to fresh applications. During the adhesion of the eschar, the best application is a common poultice, which, on the separation of the dead part, may be exchanged for any simple ointment. The sores, following the use of the moxa, are generally very tedious in healing. This, perhaps, may be attributed to the vitality of the surrounding parts being, in a measure, diminished by the application.

In some instances of deep-seated pain in bones, I have applied the cautery with advantage, so as to produce an eschar: the moxa is, however, a preferable mode of effecting this purpose. The powerful effect of this remedy is well exemplified in the following, out of many similar cases.

A man named Flanigan applied to me on account of most violent pain in his back, and stretching down his thighs. He had suffered under the disease for months, and had been very little, if at all, relieved by blisters, sudorifics, and other local and general remedies, which had been judiciously put in practice. He stated his complaints to have arisen, soon after he had been invalided, in consequence of a severe injury received on the abdomen from falling on a stake. He had been a very active, muscular man, but, from continual suffering, had become emaciated to an extraordinary degree. He mentioned, that, within a very short time, he had lost three or four stone in weight. The pains were so violent as to prevent him from ever lying down in bed; and he was generally obliged to sit up, with his knees drawn towards the abdomen. The pain was distinctly pointed out as originating at the top of the sacrum; but no change could be perceived externally, in the form of the parts. The moxa was applied over the part pointed out by the patient, as being the seat of the greatest suffering; and, in a very few days he returned, to report himself perfectly cured. Long before the separation of the eschar, he had resumed his employment—a very laborious one; and, with the exception of a slight threatening of a return of his former complaints, which occurred a few months afterwards, and which disappeared without further treatment, I have every reason to believe that the patient continues well.

I might relate many cases in which I have little doubt extensive disease in the joints was prevented by this application. When the disease which we have named Caries is fairly established, and the integuments have given way, the most rational, and, as we shall see, the most successful proceeding, is that pointed out by Nature. To accomplish our end, viz. the death of the bone, recourse must be had to different means, according to the particular circumstances of the case. We shall be the more inclined to make attempts to save even large joints, when we consider the small extent occupied by the disease in some annexed cases and plates, and the great efforts made by Nature: in many, she appears, in fact, to wait but for the separation of the sickly part, by accident or the interference of art. We shall be further encouraged by the success following the attempts in the cases to be related by and bye.

We may divide the means to be employed into those in which the disease, or part of it, is immediately removed, and those which alter the circumstances, so that it shall be thrown off by the constitution. The first indication to be accomplished by the proper use of trephines, perforators, gouges, gravers, scoops, saws, and forceps of different kinds, for dividing or extracting; the second by cauteries, actual or potential. In general, a combination of both is required.

In caries of the long bones, it becomes in general necessary to enlarge the opening through the outer lamella, by the application of the trephine, and, perhaps, by the use of a small saw or cutting forceps, so as to connect the different perforations, and thus obtain access to the diseased cancelli. The scoop or graver will answer well for the rest of the work.

Here it is difficult to ascertain, amidst the blood effused, what is sound and what diseased; and, in order to make sure, it is prudent in most instances to apply the actual cautery very freely to the exposed surface. This will by and by be thrown off, and granulations of a healthy kind fill up the breach. None of the potential cauteries are much to be trusted to; their application is equally if not more painful, and the continuance of the pain longer.

It is seldom, indeed, that any considerable exfoliation follows the use of the most active, whereas this is the inevitable consequence of the proper use of the hot iron. The application of the latter is terrible in appearance, but not in reality; and it has been almost entirely abandoned in this country—in so much indeed, that but very few surgeons have ever applied it, or seen it applied. The sooner it is reinstated in its former station among the surgical means of cure, the better, as, till then, many other diseases besides caries must baffle entirely every effort of the surgeon. Better that a patient should suffer a very little pain for a few seconds, than remain a miserable object for life (which, if the disease is extensive, will not be of long continuance), or submit to a mutilation. It will be absolutely necessary to keep the wound open, by proper dressings, until every part of the parietes granulate. The exfoliation will be accelerated, at all events the discharge, if offensive, in some measure corrected by the use of spirituous applications,—tinct. myrrh. opium, aloes, &c. or any of these combined. After the cavity becomes healthy, it would be proper to fill it very lightly with lint, so as to support the granulations.

The red precipitate of mercury, the nitrate of mercury (*eau mercurielle* of the old surgeons), lunar caustic or potass, may be had recourse to, if the potential cautery, for any reason, is preferred. The lunar caustic and red precipitate are more manageable than the *eau mercurielle*, or than any of the acids; but any of them will prove but of little use in comparison to the actual cautery. If the actual cautery is objected to, the surgeon is still bound to do all in his power towards the extirpation of the disease, seeing that Nature can with great difficulty bring it to a happy termination, unless when well assisted.

Some encouragement is held out by the first cases I operated upon, which, in the end, turned out well, though I am now convinced, that, had the actual cautery been employed, a cure would have been effected in much less than half the time, and with a great deal less suffering to the patients.

The operations on the short bones of the tarsus and carpus, are not to be undertaken but by those that are intimately acquainted with their articulations and connections, shape and size—the ligaments by which they are bound together—the direction of their surfaces—the exact point at which the tendons cross or are inserted into them—and, lastly, the precise situation of the bloodvessels and nerves. In short, the person who operates in such cases should be an *anatomist*, otherwise the most fatal mistakes may arise.

When the disease is seated in one of the tarsal or carpal bones, and entirely limited to that one, its simple removal will be sufficient. If, however, more than one (or, in some instances, two) is affected, the operation becomes more complicated and difficult for the surgeon, and more painful for the patient. Supposing (as often happens) that one of the tarsal or carpal bones is completely destroyed, and that the surfaces of those articulated with it are also more or less affected, their cartilages and outer lamella being absorbed, it is, in such circumstances, not sufficient or safe merely to remove the loosely-attached portions of the one primarily affected. The diseased surfaces or substance (if the disease extends deeply) of the surrounding ones must also be cut out, and the operation finished, for the same reasons as formerly given in speaking of caries of the long bones, by the free application of the cautery.

In caries of the distal range of bones, either in the carpus or tarsus, the ends of those supported by them are in general involved; and these, of course, must be treated after the same fashion. If one of them is diseased, with the head of the metacarpal or metatarsal bone connected with it,—and if the disease has not extended further, the removal will be sufficient, and can be easily accomplished.

In short, if the whole disease can be extirpated, leaving the surfaces of the surrounding bones covered by cartilage, the use of the cautery is not at all required, and would be productive of great harm, not to speak of the cruelty of the unnecessary application. But wherever it is impossible to avoid encroaching on the concellated texture of a bone (such as the os calcis), which it would be unsafe or imprudent to take away entirely, the use of the cautery is then indispensable. These rules of practice are fully illustrated in some of the annexed cases.

It has been often remarked, that, after the removal of the hectic cause—say a diseased limb or joint, by amputation—that the patient was immediately relieved of all complaints, and recovered rapidly. The same may be said of the extirpation of carious bones.

The symptoms disappear speedily, and the recovery is equally rapid and much more complete; in so much, that, in the one case, the patient loses his limb—in the other, he keeps it on, and is neither deformed nor lame. This could scarcely be expected from reasoning on the subject, especially on the operations of large joints, when we consider the severe and highly dangerous constitutional derangement following the slightest wound of such in the healthy state. But it would appear, that diseased joints, with or without caries, do not take on the same actions; nor does the interference with such give rise to the same disturbance in the system, as an operation on a sound joint would produce.

The instruments by which the operations are to be performed must be well chosen for the purpose, and of sufficient strength. The knife with which the incisions are to be made on and amongst the bones, requires to have a strong sharp point and edge, with a thick back and firm handle. A scoop, graver, or gouge, and strong plyers, with some pairs of cutting forceps, will complete the apparatus, not forgetting the cauteries, in case of need.

The bone forceps, the cutting edge of which is in a line with the handles, such as I have used for some years in these operations, as well as in amputation, will be found excessively useful in all cases, but more especially where the metacarpal or metatarsal bones require to be removed in part.

Forceps, somewhat resembling those I make use of, were employed by some old surgeons for dividing cartilage, a plate of such is given in the *Armamentarium Chirurgicum* of Scultetus. They were also used for removing splinters of bone after amputations, &c.; their use in cases of amputations, and in such operations as I am about to describe, I believe to be new.

Be that as it may, I have found them of great service; and my wish is, that they may prove equally serviceable to others.

It may be thought that I should say something of saws in such operations; but I never found them of the least use. On the contrary, I have witnessed several attempts on the metatarsal bones with the chain-saw. It either broke or got so wedged, that great difficulty was experienced in getting it disengaged by the use of some other contrivance, so as to put an end to the operation. The same has been the result in any trials with that instrument which I have made on the dead body.

I may perhaps have seen bad specimens of the instrument; it may perhaps, if well constructed, be found useful in dividing large bones: but the difficulty of conveying it below them must always prove an insuperable objection to its employment.

In the use of the half-headed trephine, the bone must be denuded much higher than where the division is to be made, so that the centre pin may be fixed,—the bad consequences of which proceeding I need not dilate upon; for if it is applied to that part which is to be removed, the instrument cannot be firmly fixed; and the divided end left attached to the system, will present a concave surface, with two projecting points. Another disadvantage is, that, at every turn, the centre-pin must be shifted, which, to say nothing of the delay, and increase of suffering to the patient, must give the surgeon abundance of trouble and annoyance, unless indeed we follow the plan of Mr Wardrop (the inventor of the instrument), who told me lately, on making these objections to him, that a sharp-pointed centre-pin must be used, which is to be drilled through the bone, as the division takes place. This may answer in the dead body well enough, on which alone it has, I suspect, been used.

The annular saw, so far as I can see, requires most extensive division of the integuments, and is, from its extreme complication, too apt to go wrong in working it.

An account of a new rotation-saw, as it is called, has been lately published by Professor Thal of Copenhagen. It is said to do its work well, and with rapidity. In a public trial on the dead body, amongst others, the metacarpal bone of the middle finger was divided in eighteen seconds.

With the forceps, I may merely mention that the whole operation of removing any of them (even the metarsal bone of the great toe, which is of double the thickness of the bone supporting the middle finger), incisions and divisions of the bone never occupies so much as fifteen seconds; and some of these operations may be performed in less than ten seconds.

It has been objected to my mode of dividing the bones by the forceps in the smaller amputations, that splinterings will take place, and the cure be thus retarded. This never does occur; the smooth side of the forceps is applied towards the body—the end left is as smooth as if divided by the finest saw, and appears to me to be in a more favourable state for healing.

The fact that it does heal as readily and speedily, if not more so than if the saw had been employed, must set the question at rest. The end removed is sometimes a little ragged; but this is, of course, of no consequence.

In all young patients under six or seven years, the bones of the thigh, arm, leg, and fore-arm, may be divided with perfect ease, by means of large cutting plyers, without the least splintering, and, in adults, all those of the metatarsus and metacarpus. Amputation of these latter bones has been rendered easy beyond all conception by the use of the forceps. The division of the sole of the foot, or palm of the hand, can be readily dispensed with, when the bones are thus divided. This is desirable in many instances, though perhaps the cure is not so rapid as when a free passage is allowed for the matter in both directions.

In removing the fore or little fingers, great or little toes, a flap can be saved from their sides; and it may be made either by pushing the knife through and bringing it out, or by cutting from without. The intermediate fingers or toes are removed by cutting on the back, from the point at which the division of the bone is to be made on each side, making the incisions meet near the distal end of the metacarpal or metatarsal bone on the palm or sole; the bone is then divided and turned out, leaving the soft parts untouched below.

Or a long sharp-pointed bistoury is run from the junction of the finger, so high as is thought proper on one side; it is then entered on the other side of the bone, and cut out. On securing the vessels, the edges of the wound are approximated by a firm bandage. So simple and expeditious withal is this mode of removing these bones, that the amputation of the largest or most difficult may be easily effected in from ten to fifteen seconds.

But a few days ago, the extirpation of a great toe and metatarsal bone occupied me fourteen seconds, though, from its great strength, I was under the necessity of changing the forceps I first took, up for a stronger pair. I mention this fact, to show the superior quickness with which a surgeon is enabled to put an operation out of hands (which, whatever it may be to the surgeon, is a matter of no small moment to the patient), not that I am by any means an advocate for performing operations by the stop-watch; but, consistently with safety to the patient, the more celerity that is employed the better. No man will willingly submit to quarter or half an hour's torture, if he can have the same business done, in an equally good or better style, in quarter or half a minute.

I may, in conclusion, mention, in regard to the forceps, that they have met with the approbation of many good surgeons, and, in particular, of one very great one, M. Baron Dupuytren, who has not only approved highly of them, but also used them publicly. For the details of the practice, in particular circumstances, I may refer to the cases. It must, of course, be modified by the state and extent of the parts involved.

The extent of the disease, the size of the joint implicated, and the state of the patient's health, may render the practice of removing the diseased parts only, impossible or imprudent; and for such reasons only can the operation of amputation be with propriety had recourse to.

[*To be continued.*]

VII.

On Urinary Vaginal Fistula. By WILLIAM CUMIN, M. D. Surgeon to the Glasgow Asylum for Lunatics, and one of the Surgeons of the Royal Infirmary.

PERHAPS there is no disease which, without endangering life, tends to render it more truly miserable, than that species of urinary fistula which occurs in females, in consequence of sloughing of the vagina after parturition. This injury may be caused by the pressure of the child's head in very tedious labour; but it is more frequently the effect of ill-directed attempts to deliver by means of the forceps. Such cases, I am sorry to say, are by no means of rare occurrence, particularly among women in the lower ranks of life, who are slow in applying for proper assistance. The hopeless nature of these fistulæ is generally acknowledged, and by many they are at once abandoned as incurable. M. Richerand, who deservedly ranks high among the surgeons of France, concludes his remarks on this disease in the following words. " Des ablutions fréquentes et l'usage d'une pessaire de gomme elastique, construit en forme d'urinal, peuvent *seuls* diminuer les inconveniens inseparables d'une aussi dégoutante infirmité."—*Nosog. Chirurg.* t. III. p. 545.

Although such language is but too often borne out by the distressing obstinacy of these fistulæ, there are on record several cases in which they have been successfully treated, and a cure obtained after a longer or shorter interval. The following case is, I trust, not undeserving of attention, on account of the extreme suffering of the patient, her speedy recovery, and the mode of treatment which it was judged most advisable to adopt.

H. MILLAN, a married woman, aged 22, became my patient in the Royal Infirmary, November 11th, 1818, when the following particulars were collected from her. Five weeks ago, at the full period of gestation, she was seized with labour-pains about noon, and the waters came away about an hour thereafter. On the evening of the third day, the head having made no satisfactory progress, an attempt was made to deliver by the forceps, but without success. After an interval of two hours, the instrument was again introduced, and the child extracted. Eight days after this the lochial discharge ceased, and the patient then observed that her urine escaped by the vagina. After another week, the urine continuing to pass in the same direction, a slough of one inch in length, and a quarter of an inch in breadth, was discharged from the vulva. One of the surfaces of this substance felt extremely rough, from a quantity of sandy matter by which it was covered. Since that time, her urine has continued to drain away from the vagina without interruption, attended by severe scalding pain. I have rarely seen a more pitiable object than this poor woman was at the period of her admission into the Infirmary; her clothes were so completely impregnated with putrid urine as to render her highly offensive to every one near her, as well as to herself. Sleep had nearly deserted her, and she was distressed with thirst, and with constant pain and sense of debility of the loins. The nymphæ were swollen and inflamed; and the neighbouring skin was extensively excoriated, and rapidly falling into ulceration. Her appetite, however, was still pretty good, and her general health appeared to have suffered little. On introducing a catheter into the bladder, and the finger into the vagina, the catheter was felt bare for at least an inch, through a longitudinal opening which commenced rather more than half an inch from the external orifice of the urethra. She had been under the care of a medical practitioner, who had prescribed for her some powders, but without any good effect.

November 13th.—A sponge was yesterday directed to be introduced into the vagina, with a view to absorb the urine, or prevent its escape; and, should it collect in the bladder, it was to be drawn off by the catheter as often as required. An instrument for retaining a catheter in the bladder was, in the meanwhile, ordered to be made.

18th.—The instrument alluded to was applied last night, and a gum elastic catheter introduced into the bladder. Its extremity, which was left open, was received into a dry bladder filled with sponge. Passed the night without complaint, but is this morning distressed with pain; and a considerable quantity of blood, mixed with urine, has been passed by the catheter; pulse 96; bowels slow. The catheter is directed to be plugged, with a view to allow a small quantity of urine to collect in the bladder, and thus prevent irritation from the instrument; but, should the symptoms not abate within four hours, the catheter is to be withdrawn, the abdomen fomented, and venesection had recourse to. An ounce of castor-oil; the feet to be fomented.

20th.—The gum elastic catheter has been exchanged for a silver one; by which the urine now comes freely, its extremity being left open. She feels at present easy, and comparatively comfortable. Catheter to be continued.

28th.—The catheter has been kept in the bladder without interruption; and, when it is plugged, the urine collects to the amount of about half a pint. No urine appears now to escape into the vagina. Continue.

30th.—The catheter having produced a good deal of uneasiness, it was last night withdrawn, and the instrument removed from around the pelvis. She has passed the night pretty well, and says that the urine does not now come away involuntarily; but that, to prevent this, she is obliged to void it every hour. To remain quiet, and, if necessary, have the abdomen fomented.

December 1st.—Complains only of frequent calls to void urine, which she says is discharged in the quantity of about four ounces at a time. Half an ounce of castor oil, and immediately afterwards an anodyne injection.

3d.—Pulse 64; continues to void urine frequently, and in small quantities. Last night, after receiving an anodyne injection, she slept for a considerable time, and fell into a profuse perspiration. When she awoke, she was seized with rigor, which continued for nearly an hour. Hip bath in the evening.

4th.—Frequency of making water continues. No urine comes away involuntarily, and it seems all to pass by the meatus urinarius. To bathe the vulva with solution of sugar of lead, and anoint the parts with spermaceti ointment. A blister to be applied to the sacrum, and an anodyne draught given at bedtime.

5th.—Says, that since ten o'clock this morning, she has observed her urine to pass involuntarily by the vagina. The blister rose imperfectly. Bowels open. The catheter to be again introduced, and secured as formerly.

7th.—The catheter has remained constantly in the bladder since last report, its orifice being plugged, excepting when she wished to make water. Since eleven last night, no urine has passed but through the catheter; and from that hour till five this morning, she made water only twice.

19th.—She has continued to go on well, the urine being occasionally retained, she says, for twelve hours, and then passed by the catheter. As she complains of uneasiness from the continued use of the instruments, they are directed to be laid aside.

26th.—Since the removal of the catheter, she has experienced no unpleasant feelings in any of the urinary organs; and she now voids urine by her own voluntary effort, at the usual intervals to which she was accustomed in health.

28th.—Dismissed cured, in good health and spirits, the excoriations having been for some time completely healed.

About a week after this, the patient again applied at the Infirmary, and stated to me that her urinary complaints had returned; but on detaining her in the house, and making a careful examination, this was found not to be the case.

On the 4th of May 1819, Millan called at my house to show herself. She was then in good health, without having experienced any return of her complaint; but she stated, that when she was obliged, by her situation as a servant in a small tavern, to use much exercise, running from room to room, she felt a call to make water every half hour; and when, from circumstances, this was not attended to, the urine came away involuntarily. On examination, the os uteri felt slightly prolapsed, and the upper surface of the vagina considerably puckered. A month after this, she again showed herself. Her general health was good, and she was free from complaint, excepting occasional attacks of incontinence of urine, after using much exertion. The os uteri was nearly in the same situation as when last examined; and on separating the labia, the upper internal surface of the vagina was seen protruding slightly.

In the treatment of this case, I gladly acknowledge my obligations to the writings of M. Desault, who has spoken in confident terms of the advantage to be obtained from the judicious and persevering treatment of such fistulæ. It was from this author, that the idea of the instrument by which a catheter was retained in the bladder, was derived. (*Œuvres Chirurg.* t. iii. p. 299.) The instrument which I employed, consisted of an oval steel spring covered with leather, and made so as to clasp round the pelvis and fasten over the sacrum; with straps to pass under the thighs, and prevent it from shifting upwards. In the centre, which rested on the symphysis pubis, a piece of metal was screwed on so as to admit between it and the spring a curved stalk, which could be slipped up or down, and fixed with a screw when properly adjusted. At the lower extremity of this stalk, was an aperture, through which a catheter was passed, and introduced into the bladder—then secured in its place by means of a narrow tape. In this manner a fixed point was obtained, and the motions of the catheter rendered inseparable from those of the bladder and pelvis.

Although I thus availed myself of the ingenious invention of M. Desault, I did not conceive it proper to guide the treatment of the case entirely according to the directions which that distinguished surgeon has laid down. "Dans le traitement de ces fistules," says M. Desault "on a deux indications à remplir; 1°, s'opposer au passage des urines dans le vagin; 2°, *rapprocher*, autant qu'il est possible, les bords de la division pour favoriser leur reunion." (t. iii. p. 297.) For this purpose of bringing the edges of the wound into contact, he directs a plug to be introduced into the vagina, which shall fill the tube without distending it. But it appeared to me, that the introduction of any substance so as to fill the vagina, must necessarily have had the effect of separating, instead of bringing into contact, the lips of the wound. I therefore contented myself with keeping a catheter constantly in the bladder, and allowing the vagina to collapse as much as possible. In cases of recent incised wound of the vagina and neck of the bladder, as in lithotomy, the advantage of bringing the lips of the wound into contact is sufficiently obvious, and the result of Mr Hey's case, (*Hey's Surgery*, p. 565.), evinces its practical utility. But in all cases, such as that of Millan, occurring after laborious parturition, when the new opening is the consequence of sloughing and the loss of substance, the effect of filling the vagina must be to impede, and not assist the cure. Let us figure to ourselves a tube having a portion cut out of its side, and barely filled with the most yielding materials, and we shall then at once see that the edges of the wound cannot possibly, under such circumstances, be in contact. But were such a tube allowed to collapse completely, the lips even of a pretty large wound with loss of substance, might remain in tolerably accurate apposition. Agreeably to these ideas, it seems expedient, that in attempting the cure of such urinary fistulæ, nothing whatever should be introduced into the vagina; and that our principal efforts should be directed to conducting the urine out of the bladder, so that none of it shall find its way through the newly formed opening. Nor is it of small importance that the bladder itself, as well as the vagina, should be kept in a state of comparative collapse: for which reason M. Desault has directed that the catheter should be left open, thus allowing the urine to drain off almost as fast as it is delivered by the ureters. It may be gathered, however, from the details of the case of Millan, that the coats of the bladder, when thus contracted, are liable to be much irritated by the catheter, forcing us to endeavour to confine the urine so as to distend the bladder slightly, or even to withdraw the instrument altogether.

On perusing the particulars of this case, it is impossible to avoid being struck with the rapidity of the amendment which followed, as soon as a catheter was steadily retained in the bladder. Eleven days after this was fairly commenced, no urine appeared to pass by the fistulous opening; and the bladder had so far recovered its healthy tone, as to retain half a pint of urine when the catheter was carefully plugged. The only reason which I can assign for this speedy favourable change, is the very highly irritated and inflamed state of the wound and the parts around it, at the time this poor woman came under treatment. The very sufferings of this unfortunate person were instrumental in accelerating her recovery; for the moment that the inflamed surfaces from which the slough had separated were relieved from the irritation of the urine, a healthy action commenced, and the parts began to contract and close up the fistulous opening. From this observation we may draw the practical inference, that when the edges of such fistulæ have become callous, it will be of essential benefit to apply ointment of cantharides, or some such vesicant, so as to detach completely the thickened cuticle, and dispose the parts to contract and unite.

The patient whose case has been detailed, although cured of the fistula, continued, when I last saw her, still subject to attacks of irritability of the bladder, after using much exercise. This will not appear surprising, when it is considered that the capacity of the bladder was in all probability somewhat diminished; and that the fibres at the neck of that organ had, without doubt, suffered considerable injury. Some authors have asserted, that in all cases of urinary vaginal fistulæ, when the opening extends beyond what has been called the sphincter of the bladder, a cure is wholly impracticable. But they appear to have forgotten, that the resistance of those circular fibres which close the natural opening of the bladder, is completely overcome by the introduction of a catheter; and that therefore they can have no effect in forcing the urine to find its way through the fistulous opening, which, under such circumstances, is likely to be closed by the collapse of the bladder and vagina, and by the pressure of the uterus behind. In the case of Millan, I am inclined to think that the fistulous opening extended fairly into the bladder; and, in Mr Hey's case, the wound of lithotomy must certainly have done so.

In a very deplorable case, the consequence of long-protracted and instrumental labour, which came under my care at intervals during the latter part of 1820, a large transverse slough had separated, leaving an aperture through which the finger could be freely passed from the vagina into the bladder: indeed so large was the opening, that, after some weeks, the inverted bladder protruded through the wound, and appeared, on separating the labia, in the form of a vermilion-coloured tumour, covered with transparent mucus. Yet, even in this miserable case, although treated in her own hovel under every disadvantage, by the application of blistering-plaster to the edges of the wound, and by the continued use of the catheter, the urine at length came away wholly by that instrument; the edges of the wound were found in contact, and apparently adhering; and five days thereafter the catheter was withdrawn. But two days after this, while straining at stool, she felt something give way, and, on examination, she found the inner surface of the bladder again protruding into the vagina. Had this poor woman enjoyed the advantage of being treated under more favourable circumstances, with perfect rest and complete separation from her husband, I am sanguine enough to indulge the belief that a cure might have been effected.

Besides these, I have treated two other cases of this species of urinary fistula. One of them remained for too short a time under my care to enable me to judge of what might have been the result; and in the other, which occurred during last winter, the use of the catheter produced so much irritation of the bladder, and the patient was so harassed by fears lest new injury should be inflicted on that organ, that I was forced to abandon the case as hopeless.

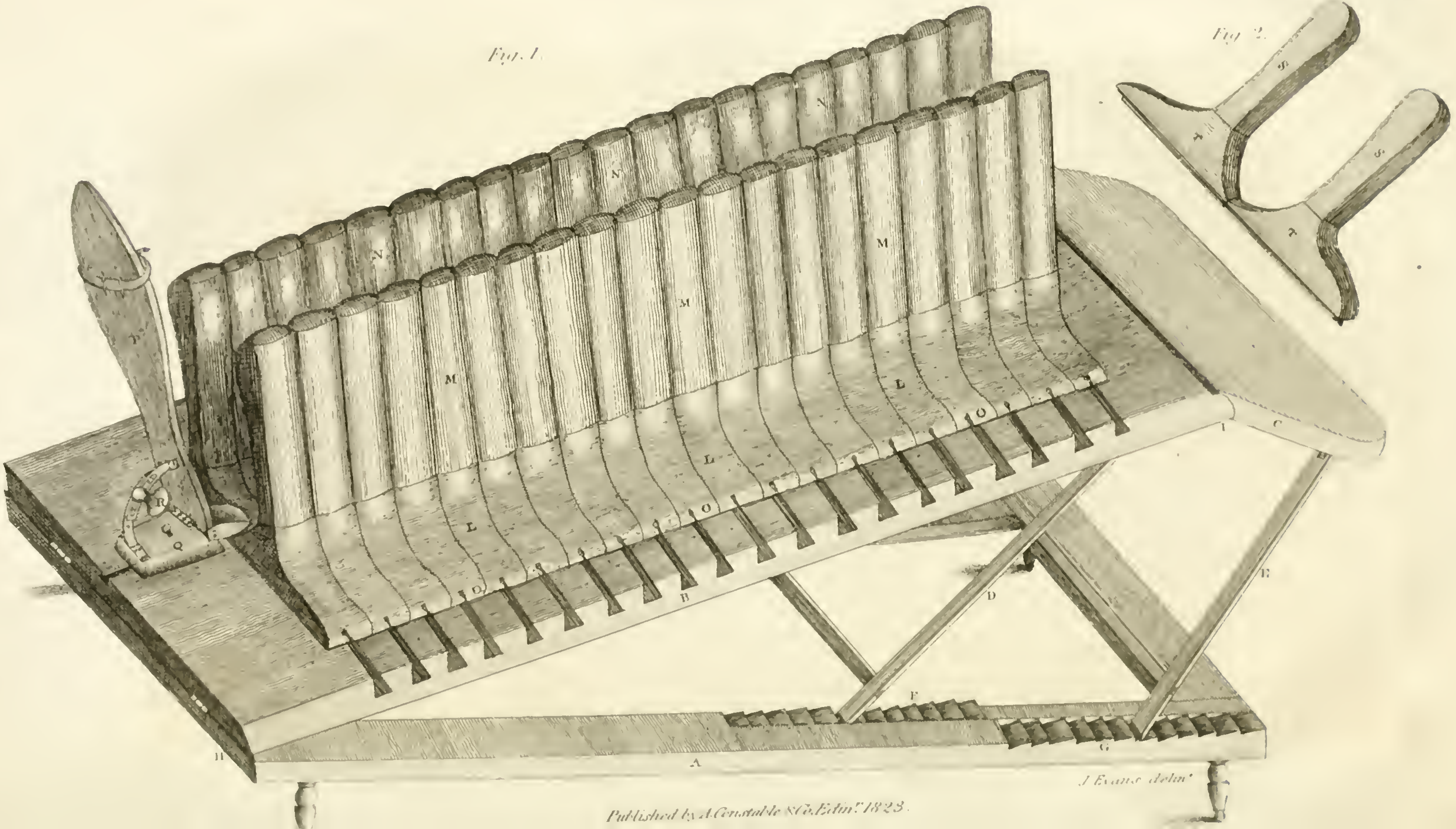
I have been indebted to the kindness of intelligent medical practitioners of this city, for the opportunity of examining two other cases of the same disease. One of them was the consequence of laborious parturition, and, I learned afterwards, fell under the care of a young practitioner, who attempted a cure by means of the suture, but without success. The other was the case of a girl of fourteen, in whom a urinary calculus had made its way, by ulceration, through the lower fundus of the bladder into the vagina, where it was felt projecting. After a slight division of the sides of the wound which embraced the stone, it was readily extracted; and was found to weigh ℈ 13 gr. 4. When I saw the case, a pretty large fistulous communication existed between the bladder and vagina, through which the urine continually drained; but the patient, from a state of great emaciation and debility, was then restored to good health. The very judicious practitioner who had first ascertained the true nature of this girl's complaint, and had relieved her from her sufferings, pressed upon the relatives the necessity of adopting some measures for the cure of the fistula, but, I believe, without any success.

VIII.

Description of an Apparatus constructed to afford Permanent and Complete Quietude in the Treatment of Compound Fractures.

Fig. 1.

Fig. 2.



J. Evans delint

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By JOHN ELDERTON, late House Surgeon to the Northampton General Infirmary.

THE consequent advantages resulting from complete quietude in the treatment of compound fractures, appear to have been received with general assent among writers on surgery, since they were first so ably inculcated in the works of the late Mr Percival Pott.

Various have been the contrivances, which ingenuity hath suggested from time to time, to ameliorate the condition of individuals suffering under compound fractures; yet, upon a general review, it must appear, that the projectors of such contrivances have been occupied more with the principle of attaining ease, and a particular position of the limb, than in the specific object of procuring a permanent state of rest, and steady apposition of the parts disunited.

In offering to the profession the subjoined explanation of my apparatus for compound fractures of the leg, I wish it to be understood as having selected such a fracture as is most frequent in occurrence, and as presenting considerations most explanatory of the treatment proposed; whereas, under circumstances less complicated, the same principle of support will be readily comprehended, as applicable to similar accidents attending some other bones.

Explanation of the Apparatus.—The letters A B C (Fig. 1.) refer to those parts of the apparatus constructed to support the fractured extremity; and are so arranged as to vary the position of the same, either to the horizontal or double-inclined plane. D E present a side view of two slight frames, attached by hinges at their upper extremities to the under surfaces of the two boards B C, and adjusted to fall into two distinct rows of notches, F G, cut on the inner and outer edge of the upper side of the frame A. From the three parts A B C having a ginglymus connection at the points H I, it may be readily understood how any declination may be given to B C, which support the leg and ham, by merely moving the frames D E in the notches F G, described above; while the horizontal position will be given to B C, by the complete approximation of the frames D E to the under surface of B C.

On the upper side of the board B, which is one foot ten inches long, and one foot two inches broad, are cut an inch apart, and half an inch in depth, twenty transverse grooves, K K, &c., so hollowed as to receive the corresponding dovetailed wooden slides, L L, &c. Each slide, which is five inches long, has a hole bored on its upper edge, half an inch in diameter, and one inch deep, for the purpose of receiving the rounded tenon of the several perpendicular wooden pins, M M, &c. (five inches long.) N N, &c. show a profile view of the second row of pins, agreeing in number, shape, and corresponding construction of slides, in every respect with those already described.

At the bottom of each transverse groove, for the space of two inches from their commencement on each side of the board B, are bored small holes, a quarter of an inch or less apart, to receive the iron pins, O O, &c., used to support the slides L L, and which, with their wooden pins M M, are to be driven more or less inward, to suit the different diameters of the fractured leg.

The letter P shows the footboard connected to Q by a single hinge. Q is a piece of wood three inches long, and two inches in width and thickness, having, on its under surface, a dovetailed construction for sliding in a groove, which runs six inches longitudinal along the centre of the board B. By means of two small quadrants running by the side of each other, and fixed singly to P and Q, together with the thumb-screw R, which passes through one quadrant to screw into the other, the footboard is retained at any elevation most suitable, while the whole foot apparatus is steadied to the length of the leg by an iron pin, which passes through the block Q into holes bored in B at short distances, in the course of the longitudinal groove. A soft leather strap and buckle retain the upper part of the foot on the footboard, the heel resting on a thick piece of leather nailed to the corresponding part of the same apparatus.

In Fig. 2. is seen a front view of the hollow designed to receive the extremity, and produced by the approximation of the double row of pins and slides. S S show the pins slightly excavated at the sides. T T the slides, so shaped as to afford, when united, a semicircular support for the leg.

On the Employment of the Apparatus.—Having adjusted the apparatus suitable to either the straight or bent position of the leg, as circumstances may render preferable, the fractured extremity is to be placed on the board between the double row of pins and slides, supported on a soft pad: a second pad is to be arranged the whole length of the limb, on the side opposite to the wound; while on that lacerated, two pads are to be employed, adjusted to support the limb above and below the part injured, and allowing an interval of space sufficiently capacious to examine or dress the wound, without disturbing either of the last mentioned pads. The slides and wooden pins are to be now driven up to support the lateral pads and limb in its natural position, and retained steadily to the various diameters of the extremity, by the small iron pins represented in the transverse grooves. The wound is to be treated after the usual methods recommended, having the corresponding slides and pins subsequently pushed up to support the dressing, and afford due pressure. The foot is lastly to be fixed on its appropriate apparatus, and raised to an easy elevation by the small quadrant behind; and, should any of the inferior slides, where the leg is short, prevent the foot apparatus from coming up, such slides are to be wholly withdrawn.

General Remarks.—If I have been fortunate enough to present an intelligible description of the apparatus in question, I trust it will appear, that some essential desiderata in the treatment of compound fractures have been supplied.

In regard to strength and steadiness of support, the slides and perpendicular pins are equivalent to the splints and boxes usually adopted, presenting, at the same time, the advantage of enabling the surgeon to dress the wound, or examine any part of the limb, without hazarding the possibility of disturbing the ossific union; and this, by merely withdrawing the slides opposite to the part under examination, or with equal facility by pulling out as many of the wooden pins as may expose the same.

Another circumstance hurtful to compound fractures, and where I conceive the above described apparatus may prove serviceable, occurs in the treatment of abscesses and sinuses which occasionally burrow in the course of muscles. Every practical surgeon must know that the prevention and cure of these states must (with the exception of counter-pressure), depend upon pressure judiciously made; but the measures commonly employed to effect local compression, by the introduction of folded linen between the splint and leg, will be better accomplished, I conceive, by giving an extra degree of pressure to the slides and pins opposite to the sinus, than in trusting to the uncertain degree of force which the above practice reasonably supposes, particularly when it is considered, that, in proportion to the local pressure we produce by such compresses, and the common splints, we leave the adjacent parts without a corresponding degree of support, and thus perhaps give origin to deviations of shape occasionally met with.

Northampton, September 3d, 1823.

IX.

Observations on Constipation, with Cases. Read before Dumfries Medical Society, 6th May 1822. By WILLIAM MAXWELL, M. D.

In compliance with the plan of our Medical Society, I have been induced to offer the following observations. Being little in the habit of composing, my time being pretty fully occupied, and my health delicate, I hope to meet that indulgence from the profession to which a desire to communicate any supposed medical facts may have some claim. It is far from my wish to deprecate reprehension; it is rather to invite criticism that I venture to offer my ideas in so slovenly a form; and that, profiting by the liberal remarks of the profession, I may, at some future period, give them to the public in a more extended and perfect shape.

The late inundation of medical communications contains many valuable discoveries and useful observations; and great are the obligations of society to the ingenuity and industry of modern medical writers. Yet a much greater debt of gratitude might be justly claimed by an individual possessed of mind adequate to the selection of useful facts and wise inductions that have been made by medical philosophers from the overwhelming matter with which science has of late been confounded. Well deserving of the highest praise are the medical men who found their reasoning upon facts; for, if their deductions be just, their superstructure will last with the world.

Modes of treatment must vary—they may improve—but facts are the only basis of truth; they are immoveable, and their value is beyond estimation. Had an individual the alternative of possessing a hundred volumes on medical theories, or a single octavo on morbid anatomy, he would not hesitate a moment in making his selection. So numerous are the causes of medical delusion, that fifty effects may proceed from the same cause, and fifty causes produce a similar effect. What a discouraging view of medical difficulties! What fixed marks have we to direct our reasonings *à posteriori* to the origin of real causes? What are we to suppose, to enable us to form a medical theory? Our progress must be the reverse of certainty. We have neither demonstration nor syllogism. Analogy and experience, I fear, are the only feeble rules of direction we possess. Such is nearly the lamentable state of medicine, the least satisfactory of the learned professions, and one which, I am convinced, no philosophical mind would select as an occupation, were he to anticipate the extent of its defects. Although the embodied exertions of genius can never render medicine a satisfactory profession, still those who are entangled in it may, and ought to, contribute to its improvement, by giving their individual observations. And it is in the hope of awakening the attention of others that I venture on this intrusion.

I have selected Constipation, or the suspension of the power of evacuating the intestinal canal, as the subject of this my first paper, chiefly owing to the very great vexation I have met with from such cases in early practice. Confining the following observations entirely to cases unaccompanied by change of structure, and dividing the subject into the following sections: 1*mo*, Constipation from indurated feces; 2*do*, From intus-susceptio, angular position of the intestine and other visceral displacements, with ileus from spasmodic contraction; and, 3*tio*, Immobility of the bowels from paralytic insensibility.

In describing the means of relief, I confine myself to those I have employed with most success. The cases are given without entering with much minuteness into the pathognomonic symptoms of the three different divisions—of which, I confess, I have only been able to satisfy myself very imperfectly, even in their early stages. I shall, in the first division, notice the very few symptoms that generally indicate its incipient progress—a costive habit; a remora of the belly, with scanty evacuations of indurated feces; an uneasy sense of fulness, generally in some part of the large intestines; general sense of uneasiness, with little or no pain on forcing up copious injections, except from a feeling of general distention; this feeling terminating at the part where the progress of injection appears to be interrupted. In this part, a lump may frequently be distinguished through the parietes. Thinking that I shall be best understood by a detail of the treatment of individual affections, I have selected the two following instances from above eighty successful cases; and in about fifty of these, the usual means of bleeding, purging, warm bathing, cold affusion, injections, antimonials, tobacco, opium, and blisters had been employed, as circumstances seemed to indicate. The first case has been selected on account of its long duration, and the well marked progress of relief.

Case I.—*September* 1804.—Mr Y. of Annan, a healthy young man, of costive habit, returned from shooting, cold, wet, and much fatigued—his bowels had not moved during two days. After a restless night, he arose unrefreshed, without appetite, feeling an uneasy sensation near the caput coli. He took a large dose of salts without effect. Third day he applied for medical advice. Bleeding, purging, warm bathing, tobacco, and purging injections were used. The uneasy local sensation became painful, extending over the belly, with a sense of contraction opposite to the right kidney. Stercoraceous vomiting, with rapid pulse, restlessness, considerable thirst, and tension of the belly. This state of symptoms advanced very gradually till the 23d day of the disease, when I first saw him. Having frequently used injections of warm linseed oil with much advantage in cases of hardened feces, the lubricity of this fluid allowing it to pass more readily than water, and its purgative quality being sufficient to excite a gentle action in the bowels, I had still some hopes of its efficacy in this very advanced case. An injection-pipe was used with a shoulder, which, pressing on the anus, prevented regurgitation. The pipe was five or six inches long from the shoulder to the thick end, on which a bladder was firmly fixed by the mouth, and thus preserved entire. It was filled by a funnel put into the small mouth of the pipe. Having introduced the small end, which was from two to three inches long from the shoulder, an assistant pressed the shoulder firmly against the anus, the patient lying on his right side, the loins raised on a pillow, while the practitioner, grasping the bag, forced its contents forward. When three pounds of the warm linseed oil had been thrown up in this way, the patient complained of pain from distension; and a sensible resistance was experienced by the operator. The oil could not be retained above five minutes, although strong pressure was made on the anus with a ball of linen—it was returned nearly pure. The enema was repeated every two or three hours, as the strength of the patient admitted. Two large lumps of indurated feces were passed with the fourth glyster, and Mr Y. thought he felt the interruption removed from the right side to the upper part of the sigmoid flexure. The fifth injection was followed by three large lumps, one of them of the size and nearly of the shape of a small pullet's egg, and these again by free evacuations.

Case II.—1818.—Thomas Gillespie, æt. 52, having had a scanty indurated stool on the preceding day, was disappointed in the usual morning relief of his bowels; and, from feeling sick, with some pain in the belly, was obliged to leave the plough. During the evening his suffering increased; and offensive vomiting became distressing. Two free bleedings were taken; calomel and colocynth were given in large quantity; but every attempt to *throw up* a decoction of senna had failed, as not a single spoonful could be forced through the pipe. By introducing the finger about two inches into the rectum, it met with a pretty firm resistance, giving the sensation of a *cul de sac*, having acted as a complete valve to the pipe. Withdrawing the finger about an inch, and directing it along the sacrum, a passage was discovered. About a quart of warm oil was thrown up before it met with much resistance. The patient was so much exhausted that the glyster could not be repeated before the evening, when it was equally unsuccessful. The pulse was now rapid. One hundred and twenty drops of laudanum were thrown up in a little warm water. In the following morning, there was a sensible improvement of the pulse, less expression of distress in the countenance, and some sleep had been obtained. The oil was again repeated to the extent of three pounds, without any sensible effect. Little change took place during the day, except in vomiting, which became less frequent. In the evening, the oil was again repeated to the extent of two quarts—the patient standing on the floor, and leaning his chest upon the bed. No material change occurred in this day. A few indurated lumps were passed in the oil of the second injection of the third day, and the succeeding night was more comfortable. In the morning of the fourth day, some hardened lumps came off with the injection, and during the day there were copious evacuations.

It is a mistaken idea, that a valvular structure at the end of the ilium, and beginning of the colon, can prevent the progress of injections. In these parts the bowel is only a little thickened and contracted. In a healthy state of the intestines, warm water may be made to pass upwards into the stomach. When a large quantity of fluid is forced up by injection, the patient should be placed in a posture that will allow the bowels to hang at nearly right angles with the spine. About fifteen years ago, in a case of colic, from which Mr H., a master gardener, had suffered for twenty-four hours, I threw up three and a half gallons of warm water, accurately measured, before it reached the stomach. In a healthy state of the intestines, I believe that, by attending to posture, any quantity of water may be made to pass from the anus to the mouth.

Having found, by dissection, in several cases that had been supposed intus-susceptio, that the mischief had been occasioned by an angular portion of the bowel stopping the progress of its contents, and that, when the two surfaces adhere, which frequently very soon takes place, there is very little chance of replacing it either by purgatives or watery injections, I thought that the bowel might probably be restored to its natural situation by inflation; and believing that the same treatment would apply to intus-susceptio and spasmodic affections, I remained on the look-out for such affections during some years. It was from having failed in relieving a young lady from England, who died in the King's Arms Inn here about ten years ago, that I first thought of inflation—being sensible that something beyond the power of injection might occasionally be required.

Case I.—The first case that occurred was that of William Irving, æt. 43, in 1817. After performing the operation above the ring for strangulated hernia of three days standing in the abdominal canal, the bowels did not move by previous cathartics, or by injections, subsequent to the operation. A piece of cork, nearly flat, was placed on a male catheter, at three inches from the point, which was introduced into the rectum. The patient being placed on his back in bed, and the cork pressed firmly against the anus, the bowels were gradually inflated, the top of the catheter being stopped with the tongue during inspiration. When the air had occasioned considerable distension, it was readily expelled, but nothing followed. Pondering upon my disappointment, it occurred to me, that the sudden distension of the colon might press so much upon the ilium, that the air could not enter it. I resumed the operation, blowing slowly, and with the left hand pressing the air forward along the colon into the ilium. When the distension began to give much pain, the air was allowed to escape, and, in about an hour, it was followed by copious soft stools. The patient walked out on the fifteenth day after the operations.

Case II.—1818.—Mr B.'s child, æt. 4, had been constipated during nine days. Cathartics, injections, warm-baths, were ineffectually employed. The belly was inflated without relief; the operation was repeated; the air was pressed forward in the course of the colon as in the last case, and was soon followed by free evacuations.

Case III.—1818.—A child of Mr Y. at Castle-Douglas, æt. 5, in hooping-cough, had been without stool for six days. Owing to its excessive impatience, the bowels were very imperfectly inflated. They moved freely during the night.

Case IV.—1818.—The child of Mr F. in Dumfries, æt. 10 months, had been without relief in the bowels for six days. The bowels were inflated at nine *p. m.*, and moved freely at one next morning.

In these last three cases, purgatives, with enemata, had been given freely, with blisters and frequent warm-baths. The above treatment has been invariably used with success in seven other cases. During the inflation, the patient must lie on his back, and the distension be made gradually, pressing the air along the colon into the ilium. Unless these circumstances be attended to, the operation will not succeed.

The third cause of constipation is of more rare occurrence; it proceeds from such a degree of paralytic torpor as renders the bowels insensible either to the stimulus of cathartics or injections. I have selected the two following cases.

Case I.—1803.—Mr C., æt. 61, after much fatigue and intemperance, and anxiety of mind, had an attack of inflammatory fever; and, although depletion had been very actively employed, symptoms of palsy began to appear below the neck; and, in spite of purgatives and injections, the bowels remained immoveable, and the belly tense, without any stool for seventeen days; but little uneasiness was felt in the abdomen, even on hard pressure being made. An electrifying machine being procured, three shocks of the charge of a two pound vial were made to pass through the belly without being felt by Mr C. He thought he was sensible of the fourth, and complained of their becoming successively stronger. After fifty had been given, the ten following occasioned considerable pain; he then felt a disposition to have a stool; his bowels moved freely, and Mr C. recovered.

Case II.—1815.—Mr H. a master mason, æt. 50, of a full habit and large head, had an attack of inflammation in the head, which was removed by purging, general bleeding, and by taking repeatedly large bleedings from the jugulars. His health and strength were reestablished. One of his legs, however, could be perceived to drag a little, but in so slight a degree that he was not sensible of it. In 1816, he had a return of the pain in his head, and, nothing being done for some days, a slight attack of palsy, similar to the former, affected him. The evacuating plan of the preceding year removed the headach; but the power of his limbs continued much impaired. The state of the stomach was remarkable. When four ounces of any fluid were taken, it was immediately rejected; a smaller quantity remaining in the stomach until it accumulated, was then vomited as if it had been swallowed at once. This happened frequently from the accumulation of saliva, which was secreted in very unusual quantity. The stomach appeared to have lost sensation. An ounce of spirit of turpentine, with an equal quantity of aromatic tincture, was given, without producing any sensation different from that occasioned by watergruel. The bowels could not be moved, nor was any urine secreted. Electricity was used as in the preceding case of Mr C., without being felt by the patient. It was repeated to the extent of sixty shocks, at each of three times during the first day. A natural stool was passed in the evening; a considerable quantity of urine was evacuated during the night; but the stomach still remained the same, although the electric shocks were made to pass through it in all directions, without making any uneasy impression. The life of our patient was protracted fifteen days longer by broth and opium injections. Mr H. appeared to sink entirely from inanition; all internal medicines and external applications were equally unavailing.

I shall take the liberty to remark in this place, that endeavouring to prevent vomiting in cases of obstructed bowels must accelerate fatal symptoms. It is by an inversion of the intestinal action alone that the distended bowel can be relieved from the pressure of its contents, till the impediment be removed. On inspecting the body of Mr D., a gentleman who died after the operation for hernia, it was evident that vomiting, with inverted action of the ilium, would have saved him, by removing the immense quantity of fluid in the bowels, which had pressed them to the bottom of the pelvis. Since this unfortunate case, I generally promote vomiting, even in cases of incarcerated herniæ, although the inverted motion can almost to a certainty be prevented. The mode of obtaining this object will probably be the subject of a future communication.

Dumfries, 6th May, 1822.

X.

Case of Inflammation of the Spinal Cord, combined with Inflammation and Suppuration of the Right Kidney; with the Appearances after Death. By William Jones, M. D., Surgeon, First Dragoon Guards.

The following case, with the appearances after death, affords an example of a complication of disease not very frequently recorded in medical writings, and, perhaps both in a practical and pathological view, is not unworthy of being more generally known than it would be if confined to the pages of a regimental Journal. I am also not without hopes that the publication of this may draw the attention of medical officers, similarly situate as myself, to the subject of diseases peculiar to the cavalry service, and eventually show, that the occurrence of which I now present an example is not solitary, or very unusual in subjects favourable to its formation.

February 25th, 1823.—BENJAMIN HOPKINS, æt. 20, of sanguine temperament, native of Leicestershire, by trade a stocking-weaver, was attacked, a few days ago, with pain in the region of the right kidney, which is now become acute, and extends, he says, to the chest. He has no cough, but the pain is increased in every action of the diaphragm; pulse 108; skin warm; tongue coated; urine high-coloured; countenance pale, and expressive of suffering from pain.

> *Capiat quamprimum pilul. hydr. subm. gr. v. et sulph. sodæ solut. unciam 2dâ quâque horâ donec bene responderit. Venescctio ad ℥xxiv.*

Vespere.—Pain less acute; seven stools from the purgative. *Capiat pulv. Jacobi gr. vi. h. s.*

26th.—Blood drawn last night is covered with a thick buffy coat; return of pain increased on each inspiration; tongue much coated; pulse 120; headach, with thirst.

> *Repet. Venesectio ad ℥xx. Capiat antim. tartarisati solut. unciam, 2dis horis.*

Vespere.—Nausea; headach lessened; pain returns occasionally.

> *Contin. Pulv. Jacobi horâ quâque 4tâ. Descendat in balneum calidum.*

27th.—Pulse 120; pain of loins returns occasionally, extending along the course of the ureter, with numbness of the thigh, which extends to the knee; felt much relief from the bath; only one stool since yesterday morning.

> ℞ *Hydr. submur. gr. v. Pulver. antim. gr. vi. Extr. colocynth gr. x. M. et divide in pilulas octo. Sumat duas horâ quâque 2dâ donec alvus respond.*

Vespere.—Two alvine evacuations from the pills; general diaphoresis; pain much relieved.

Vespere.—℞ *Pulv. ipecac. comp. gr. vi. Pulv. Jacobi gr. iij. M. h. s. capiatur.*

28th.—Pain returned this morning, extending around the course of the ureter, with retraction of the testicles; has pain also along the right thigh to the knee. On getting out of bed this morning, fell, being unable to support an erect position; has had two stools since last night, but has passed no urine since noon yesterday; fulness of the hypogastrium; pulse 120, small; anxiety.

> *Educatur urina, admoveatur cucurbitulum cum ferro parti lumborum dolenti. Repet. balneum calidum.*
>
> ℞ *Ol. ricini ℥i. Mucilag. acaciæ q. s. aq. menthæ ℥vi. Spt. æth. nitr. ʒij. M. Capiat uncias duas horâ quâque 2dâ.*

Vespere.—Near two quarts of urine drawn off at ten this morning; has been easy since he came out of the bath.

Contin. mistura.

March 1.—Pulse 108; skin and tongue moist; several stools during the night; slept little; has passed no urine since last night; can stand to-day, but complains of numbness down each thigh, extending to the knees.

Educatur urina. Repet. mistura ter in die.

Vespere.—About three pints of thick high-coloured urine drawn off this morning; has had several fluid evacuations, but has passed no urine since the morning.

Educatur urina.

Hora 12ma, *p. m.*—Was seized, about an hour ago, with severe pain, extending to the right breast and scapula; pain increased on each inspiration catching him, as he describes; pulse 120, with hardness.

Repet. balneum et V. S. ad ℥xvj.

℞ *Liquoris Ammon. Acet. ℥ij. Liquoris Antim. tartar. ʒij. Tinct. digit. ʒj. Tinct. opii gtt. xxx. M. Capiat unciam horâ quâque 2dâ.*

March 2d.—Blood drawn last night cupped; pulse 120, easily compressible; has had no stool, or passed any urine, since the use of the bath last night; passed a quiet night, with some sleep and freedom from pain.

Educatur urina. Cont. mistura sine opii.

Vespere.—Has been easy the whole of the day, but has had no evacuation from the bowels.

Injic. enema purgans.

3d.—Two stools during the night; has passed his urine without the catheter; pulse 114, soft; feels free from pain; coughed once or twice during the night.

Cont. mistura.

Vespere.—Had an exacerbation of fever about four this afternoon; skin hot; thirst urgent; cough more frequent; pulse 120, small and hard.

Contin. mistura. V. S. ad ℥x.

March 4th.—Has had three fluid evacuations since last evening, and passed about three pints of urine; pulse 128, small; perspired much during the night; left cheek flushed; complains that he had a return of pain of the back, and numbness of the thighs to the knees; cough more frequent; thirst and dyspnœa.

℞ *Hyd. submur. gr. i. Pulver. digit. gr. i. Opii gr. ¼. M. ft. pilula 3tia quaque hora. Sum. in dosi misturæ sine tinct. digit. et opii. Empl. lyttæ inter scapulas.*

Vespere.—General diaphoresis; expectorates with ease; pulse 124, more full; has passed some high-coloured urine twice.

5th.—Blister rose well; slept a little during the night; expectorates with ease; general moisture of skin; pulse 124, full; tongue moist; dyspnœa lessened; has had one stool, and made water twice; complains of some numbness about the small of his back; ate some toast with his tea this morning.

℞ *Infus. colombæ ℥j. Acid. sulph. dil. gtt. v. M. ter die sum.*
Some wine and sago frequently during the day.

Vespere.—Pulse 130; dyspnœa more urgent; complains of uneasiness, which he refers to the back and inferior part of the right hypochondriac region.

Cont. pilulæ hyd. submur. et digit. 3tia quaque hora.

6th.—Passed a restless night; took his sago and wine frequently; pulse 126, small; general moisture of surface; three stools during the night; has passed about three pints of turbid urine; complains of pain, which he refers to the inferior part of the right hypochondriac region.

Applic. empl. lyttæ parti dolent. et cont. pilulæ.

Vespere.—Dyspnœa urgent; pulse 130, small; one partial stool of spinach-like appearance; great debility.

Rept. enema. Wine and sago to be continued, with a little jelly.

7th.—Took his nourishment frequently; mind wandering; passed a restless night; dyspnœa more urgent; pulse at wrist imperceptible; moribund. Died at 12, *a. m.*

Appearances on Dissection of Benjamin Hopkins.

On opening the cavity of the chest, extensive adhesion had taken place between the pulmonary and costal pleuræ of the right side, by effusion of a quantity of coagulated lymph disposed in sacculi, soft and lacerable, as if recently effused. In the cavity of the abdomen, the liver was found unusually large, but without marks of organic disease. The cellular texture surrounding the right kidney was highly vascular; and, in the substance of this kidney, were two spots of purulent matter, each about the size of a small pea. At the inferior part of the lower dorsal vertebræ, on the right side, an abscess about the size of a walnut was discovered, of recent formation. A still smaller abscess was found in the same situation on the left side. On sawing through the vertebræ to expose the spinal chord, the theca was found covered by a multitude of red vessels; it was much thickened, and dotted with purulent matter on its surface. On opening the sheath, the same degree of vascularity was evident on the cord, the vessels being turgid and tortuous, but seemingly no disorganization of the substance of the cord. The preparation has been transmitted to the collection of morbid anatomy at Chatham.

When this patient was first brought to Hospital, the great anxiety and paleness of countenance, indicative of bodily suffering, induced me to make instant inquiries of the Sergeant-major, whether his complaints were of long continuance, or whether he had been engaged in some act of intemperance the preceding evening. In answer to these inquiries I was informed, that he continued in the performance of his duty until the night previous to his admission into Hospital. I was indeed undecided whether his complaints were to be referred to acute rheumatism, inflammation and abscess of the loins, or inflammation of the kidney. The obstinacy of the local uneasiness, without being affected by external pressure, and the continuance of general fever, notwithstanding the active employment of antiphlogistic remedies, showed in a short time that it was not to the first of these affections that the symptoms were to be ascribed; but, previous to the 28th of February, the third day after admission, and perhaps the eighth or ninth day of the disease, the phenomena which Hopkins presented were not sufficiently pathognomonic to determine the precise seat of the disorder. At this time, however, the return of local pain, and its extension in the course of the ureter, with retraction of the testicle and retention of urine, indicated disease either of the kidney or ureter; while another symptom, his falling from the erect position on getting out of bed, denoting either fainting from loss of blood, or loss of power in the muscles of the lower extremities. The eduction of the urine by the catheter, from the 28th February to the 3d March, showed that the secretion had not been suspended, and that it was retained in the bladder in consequence of want of expelling power. On the following day, March 4th, when this had disappeared, a different symptom took place, viz. numbness of the back, thighs, and knees, and continued till his death, on the 7th. But although this variation in the symptoms certainly led me to doubt whether the only cause was inflammation of the kidney, they were so obscure and ambiguous, in consequence, perhaps, of being complicated with other affections, that it was difficult to form a decided opinion, and thus might have remained involved in obscurity, had dissection not afforded a most satisfactory explanation. We learnt from this, that two morbid actions, inflammation and suppuration of the kidney, and inflammation of a considerable portion of the spinal cord, had existed at the same time, and had thus, in some degree, concurred to produce the unusual combination of symptoms which took place. Another useful conclusion however is, that a considerable portion of the spinal cord may be inflamed without giving rise to very obvious signs. The practitioner, whenever pain is referable to any part of the spinal cord, should use every means to counteract the disease. In the present case, the only signs of the affection consisted in the loss of muscular power in the extremities and bladder on the 28th February, and the numbness of the back, &c. on the 4th March; yet the bladder recovered the power very nearly at the time when the numbness appeared; and many of the other phenomena might have arisen from a concretion sticking in the ureter. Notwithstanding this obscurity, it is interesting to remark, that the symptom of numbness corresponds accurately with the conclusions derived from some recent experiments on the function of the anterior and posterior bundles of the spinal nerves. It is well known that Majendie has shown, that the anterior spinal nerves are connected with muscular organs, and subservient to motion, while the posterior are connected to the skin, and subservient to cutaneous sensation. Now the examination of the spinal cord showed, that though the sheath, and the cellular substance between it and the inside of the vertebræ, was most inflamed at its anterior part, the enlarged blood-vessels exercise most pressure at the posterior and lateral parts; a circumstance which explains at once the trifling affection of the loco-motive organs, and the numbness of the back especially.

With the view of obtaining information on the probable cause of this combination of disease, I have, since the death of Hopkins, made some inquiries about his previous history, and have learnt from his comrades that he frequently spoke of pain in the back, though it never appeared to have been so severe as to lead him to complain of it to me. In the cavalry service, there are various causes which may excite similar disease in the persons of those otherwise predisposed, and especially among young subjects, who have not attained the maturity of growth and strength. Among these may be enumerated the practice of hard trotting without stirrups, a system of drill necessary to make a good horseman, great exertion of the lumbar muscles in the sword exercise, which requires the twofold object of retaining the seat in various movements, and at the same time wielding arms of considerable weight. If these conclusions be well founded, they show the necessity of attending to the earliest symptoms of uneasiness in young dragoons, and in every fatal case which may be even moderately suspicious, of examining the state not only of the spinal cord, but of every part which is likely to be affected by injury in unusual efforts.

Notwithstanding the obscurity of the exact seat of this disease, its inflammatory nature was sufficiently manifest; and I had, therefore, no doubt about the kind of treatment, and the extent to which it was requisite to carry it. The only circumstance, perhaps, which can with any justice be said to have been less rigorously enforced than I could have wished, was the local detraction of blood; for the general bleedings were carried as far as was consistent with the powers of the patient. Hopkins, indeed, was cupped on the 28th; but had it been repeated, or its effect favoured by the application of twelve or eighteen leeches, at two or three separate times, the progress of the disease might have been arrested, or its violence moderated. It may be doubted, however, whether this would have prevented the fatal termination. His death seems to have been occasioned partly by general exhaustion, partly by the disease of the lungs, and, perhaps, by the morbid action being con-considerably advanced before it led him to apply for relief.

XI.

Letter to John Thomson M.D. Edinburgh, containing Observations on the Occurrence of Small-pox after Small-pox and Vaccination. By WILLIAM STOKER, M.D. Dublin. Communicated by Dr Thomson to Dr Duncan Junior.

DEAR SIR, *Dublin, October,* 1823.

DESIROUS to avail myself of an opportunity of expressing my sense of the great value of the Jennerian discovery, and my respect for its author, as well as to conciliate public opinion by the sanction of his name, I dedicated to him the little Treatise, which contained my contribution in aid of the very anxious and general inquiry, set on foot a few years past, by those who like myself, deemed it a duty to investigate the alarming reports then in circulation, of frequent failure in vaccination, to give security against the contagion of small-pox. Convinced, however, by all my observations, and especially by those which I have made on that subject within the last nine months (a period during which variolæ, and some other of the sporadic diseases of this country have been epidemic), that the value of Dr Jenner's discovery, to be duly estimated and firmly established, requires still further investigation, I beg leave to address the following letter to you, who have, in your inquiry and publications on the nature and extent of the security afforded by vaccination, evinced so active an interest for its success.

In this letter, it is my intention to state some facts which latterly came under my notice, and appeared to me well calculated to assist in establishing just principles and fixed opinions with respect to vaccination, so very desirable on a subject of such deep and general interest; and conceiving that it may be useful in that way, I am very desirous that it should be published with such a mark of your sanction, in any of those respectable Journals that issue from your city, or in such other way as you may be pleased to direct.

With permission, I beg leave further to preface the cases with which I intend to illustrate some observations on secondary small-pox, by remarking, that the opinions expressed in my Observations on the varioloid disease published in 1821, were formed, without any previous knowledge of what you had written on the subject, particularly with regard to the identity of the origin of the various pocky exanthemata; but in making this remark, it is obvious I cannot mean to interfere with the priority of your opinions, but deem the statement interesting, in as much as the unbiassed coincidence, which took place in our deductions from similar facts observed in places remotely separate from each other, adds presumptive evidence in their favour, and, at the same time, affords me an opportunity of stating, that had I been aware of your authority, I would most satisfactorily have referred to and relied on it.

Case I. Miss ——, resident at Rathmines near Dublin, 22 years of age, had been generally healthy excepting slight bilious complaints, to which she had been sometimes disposed, and those which will be afterwards mentioned, until Monday the 28th July 1823, on which day she was suddenly and violently seized with fever, accompanied by præcordial oppression, mental anxiety, loss of rest, and severe pain in the back and loins. For these symptoms, leeches to the temples and active purging had been employed, under the direction of surgeon Ryall.

On my first visit, the 30th July, fever was extremely urgent, accompanied by moaning, anxious countenance, total loss of rest, hurried respiration, impeded by spasmodic pains in the stomach, tending to the back and left side of the thorax; frequent jactitation of the body and limbs &c.; slight delirium; pulse 140, full, but unequal and irregular; tongue loaded and brown, but soft; catharsis urgent; skin hot in general, but rather cooler than natural on the feet; thirst still urgent, though somewhat abated since diaphoresis commenced. A general blush, most vivid on the face and neck, was diffused over the whole surface, and, on closer examination, a thick eruption of minute red points (such as Sydenham designates the first appearance of small-pox) was then beginning to show itself. Her parents however stated, that at six months old she had been inoculated by Mr Sowan with small-pock matter, which was then succeeded by pustules on various parts of the body, and which went through their usual course. A bad sore, however, succeeded the puncture in the arm, which was a long time healing. There is now a deep and extensive cicatrix on the arm which had been so affected. There are many broad but not deep pittings to be seen, especially on the back of the neck, left by the swine-pock, which this patient is stated to have had very severely. About eight years ago she also had measles and scarlet fever. *Prescribed.*—The head to be shaved and washed with camphorated spirits of wine, a blister to the back of the neck, and the feet to be fomented by flannel and hot water; an anodyne draught at night; barley water for drink.

Second visit, 30th July.—A very restless night, but some sleep towards morning. The eruption is now elevated with deep-coloured bases. It is chiefly vesicular, but in many places pustular. It is confluent on the face, and the discoloration extends considerably over the cheeks, which are somewhat swelled; and it is diffused very generally over the trunk and extremities. No catharsis since the anodyne draught was taken. The pain of head, stomach and back, easier; pulse 120, and regular; the renal secretion scanty, crude, and turbid. *Prescribed* an enema in the evening, and eight grains of anodyne powder at bedtime.

August 1st, report.—Some rest, and feels better to-day; pustules considerably increased in extent, and their inflammatory bases and the interstices between them of rather a higher colour, accompanied by a considerable swelling of the fauces. Some griping, and the alvine discharges scanty, and greenish. *Prescribed*—An aperient absorbent mixture, and the anodyne powder to be repeated.

August 2d.—Slept well, and feels better; pulse 100, bowels free; pustules more fully maturated, many of those on the face having a depression in their centres.

On the three succeeding days fever declined, and the pustules rapidly desiccated, and the scabs fell off much sooner, and left the parts underneath of a lighter colour than I have witnessed in other cases of varioloid disease. Considerable delicacy of health however remained, and tendency to slight fever in the evening, for nearly a fortnight afterwards.

CASE II. PATRICK BUCKLY, five months old, infant of a patient in Cork Street Hospital, was (with its mother's permission) inoculated at my request by Mr Hall the apothecary, with matter which I took from the pustules when fully maturated in the preceding case. The matter was inserted on the child's arm on the 4th August; aperient and alterant powders prescribed at the same time. On the 12th, the punctures exhibited signs of the new action produced by the virus introduced; and on the 13th, two distinct vesicles were formed on one puncture, and three on the other; and on the succeeding day these vesicles were farther distended and pustular, unaccompanied by any unfavourable symptoms, either local or general.

From these pustules I took matter, for the purpose of further inoculation, but have been deterred from employing it, from the unfortunate sequel of this case; for the mother of the child having been herself for some days convalescent from fever, could not be restrained from returning home many miles from Dublin, and taking her child (which she suckled) along with her. On the 22d August, she again presented herself at the gate of the hospital for admission, having relapsed in fever, probably from the fatigue of her journey, with the child also in a high degree of fever, and covered over with the confluent pustules of small-pox. Both being placed in a ward under the immediate care of my colleague Dr Harkan, the symptoms went on favourably for several days, the mother becoming convalescent on the 5th day, and the child's fever gradually subsiding until the 2d September, when he was seized with convulsions, with which it seems he had been frequently attacked, having commenced soon after his birth. On the present occasion, they were suddenly fatal. Although I conceive that the injurious effects of the journey on the child and its mother, did in this instance materially influence the succeeding course of the disease; and though possibly the epileptic attack might have been fatal, if the small-pox inoculation had not taken place, yet the event must be deemed interesting, especially in connexion with the succeeding cases, which occurred in the brothers and sisters of the young lady from whom the matter was taken. Although vaccinated, they caught the disease from her without inoculation, and had it in a much milder degree. It is also worthy of remark in this place, that surgeon Rumley of French Street, who, at my request, took matter from the pustules in the first case two days before I did, informed me that he inoculated two children with it, one of whom had been vaccinated, and another with whom no antidote had been employed; and that in neither case was there any other consequence from the inoculation, than would be produced from a similar wound from a clean lancet. The child not vaccinated, however, was attacked with measles in a few days after inoculation, and that disease went through its regular course, and was not succeeded by small-pox.

Four cases of small-pox after vaccination, or those of two brothers and two sisters of the young lady whose case is given above, may all be given under the same general description. Their ages were 12, 14, 16, 18 years,—all had been vaccinated by Dr Tuke, and all had that cicatrix which is generally deemed the mark of having undergone complete antidotal process of vaccination. Three weeks nearly elapsed between the sickening of the first two of these, and the convalescence of their elder sister, and nearly a fortnight more before the two last sickened.

The eruptive fever was severe and urgent in all of them, the eruption appearing on the third day; and all had numerous pustules fully maturated and desiccating, and also in some of them leaving slight pitting, as in small-pox. There was no secondary fever in any of them; they all were convalescent on the seventh day, and did not require any medicine, except a mild aperient.

I have selected the foregoing cases, as they appear to me well calculated to illustrate some observations which I propose making on the remarkable frequency of secondary small-pox, during the epidemic prevalence of that disease in Ireland during the last nine months; a frequency which has been observed by many of my medical friends as well as by myself, some of whom I shall mention in the order in which I conversed with them on this subject.

My friend the Surgeon-General, Mr Crampton, who had long before witnessed the supervention of small-pox after vaccination, and the modifications of that disease thus produced, informs me, that he has seen several cases of it during the current year, in some instances severe in its course, and extending by contagion through families, but more generally mild in its symptoms, and without any secondary fever. In one instance, the patient had had small-pox in a very severe form from inoculation, 28 years before.

Dr Mills saw two cases of secondary small-pox; one, that of a young Baronet, which he saw in consultation with Dr Graves, it being a case of great urgency, though the patient had been vaccinated, and subsequently inoculated with small-pox; the other was a young gentleman, a student in Trinity College, who had been also vaccinated, and in whom also the symptoms of confluent small-pox were extremely urgent.

Dr Colles saw several cases of this kind; in one of whom, a young lady, the symptoms of confluent small-pox, after regular vaccination, were very alarming, until the eleventh day, when they suddenly subsided, leaving, however, very deep and extensive pitting over the face and surface of body; in the other, a young gentleman of adult age, the patient was carried off by the violence of the eruptive fever on the second day.

Dr Cheyne, the Physician-general, has lately seen many cases of small-pox after vaccination, and some cases even after small-pox inoculation.

I might add many more respectable testimonies to the foregoing; but deem these sufficient, along with those which I have adduced in my Observations on the varioloid disease, published in 1821, to prove the frequency of secondary small-pox in latter years, and especially since the introduction of vaccination,—an occurrence so very rare formerly, that Dr Tissot, writing in 1772, and doubting that it ever happened, adds, "Dr Monro, in his late extensive inquiry upon this subject, has not, among all the practitioners of Scotland, been able to produce a single instance where the disease has been twice received; but, on the contrary, detects the fallacy of several such histories as had been offered to him."

The observations I have to make on the cases which I have adduced, refer chiefly to the course of the symptoms in the case of the young lady first detailed, being in many circumstances similar to what has been observed when small-pox extends to those who had been previously vaccinated; such, for example, as the diminished size of the pustules, but particularly the absence of secondary fever,—circumstances which corroborate the testimony of the parents and friends of the young lady, of her having previously had the disease.

Of its being genuine small-pox, the result of the inoculation with the matter taken from it, leaves no room for doubt; but it is very remarkable that so severe a disease was produced by the inoculation of matter from it, when it is recollected that the M'Cleods, whose cases I published in my Observations on this subject, had it in a modified form, from having caught it from those labouring under it after vaccination, although they themselves never had been inoculated with either variolous or vaccine matter.

The occurrence of an ill-conditioned sore on the part where the puncture was made in inoculating with small-pox, in the first case detailed, is worthy of remark in this place; as perhaps it was on this account (as happens with vaccination), that its complete and antidotal influence was interrupted.

Such facts and observations, it appears to me, may be usefully applied in estimating the antidotal efficacy of vaccination, since it is found, that during the prevalence of small-pox epidemically, as happened in the latter end of 1820, and beginning of 1821, and also during the last ten months in this country, cases of secondary small-pox have been met with in extraordinary frequency; and, in the same periods, the protection by vaccination is also stated to have very frequently failed. It would appear also, from the first case reported here, as if morbid actions of the part inoculated interfered with the antidote, and farther evinced the similarity of their natures.

With respect to the *constitutio epidemica*, or that increased predisposition in the constitution to be affected by diseases at particular periods, though it might be more astute to acknowledge the inscrutable difficulty of the subject, I cannot help adverting to an observation which occurred to me when lately tracing the course of the influenza,* which prevailed for some time past in this country, combined with small-pox and other sporadic diseases, that the animal fluids being then very generally (but especially the blood itself) found altered, it is at least probable that this *constitutio epidemica* consists in that change of the fluids produced by certain states of the weather, previously to which, small-pox inoculation or vaccination would have afforded a sufficient protection.

That the fluids are primarily and chiefly engaged in small-pox, is an opinion I conceive well established by the observations and experiments of Dr Walker, in his able Inquiry into the Small-pox, published in 1790,† although his experiments may not have afforded the full evidence in favour of his chemical theories which he expected. It is here, however, to be observed, that the varioloid disease, or that form of small-pox which has been latterly so often noticed as occurring after vaccination, although modified, yet is not so essentially changed from the primitive form, as the others of the pocky exanthemata, to which the antidotal process of inoculation and of vaccination still leave the human body susceptible, seeming to arise from some disturbance in the antidotal process, from causes such as were stated in the first case detailed here, and such as are found to act in a still more remarkable degree in cases of vaccination.

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* See Pathological Observations on Dropsy, Purpura, &c. &c. Page 209. Dublin, 1823.

† See an Inquiry into the Small-pox, Medical and Political, &c. By Robe Walker, M. D. Page 75. London, 1790.

That the disease still preserved the genuine characteristics of small-pox, was evinced in the first case related here, by the inoculation with the matter taken from it having produced an exquisite and severe form of that disease; and in the cases of the M'Cleods, which I reported on a former occasion, which went through the small-pox in its modified form, from having caught it from those who were affected with it after vaccination, and yet were found protected; so that inoculation with small-pox or vaccination, subsequently tried with them, had no effect.

Being of opinion that the varioloid disease which has been of late chiefly observed, is, as well as the other pocky exanthemata, derived from genuine small-pox, the common origin of them all, I am inclined to hazard another conjecture, that it is only the peculiar virus of each which can give protection from the various forms which they assume, after having once emanated from the parent stock,—*forms* which characterize them in each future transfer *ad infinitum.*

The laws of propagation by the seeds of vegetables, afford some illustration of these phenomena in the *semina* of diseases. Take (for example) the potato, the *solanum tuberosum*, to us here at least a homely example. If its seeds, taken from the perfect form of the plant which produces them, shall be sown, almost every variety will be found to grow from them; but let these be propagated either by cuttings or shoots, and they will always preserve exactly similar forms to those from which they were propagated.

The practical corollary that may, I think, be fairly and usefully deduced from such facts, to which, in concluding this letter, I beg leave to call your attention, consists chiefly in the increased evidence of the antidotal efficacy of vaccination, in a very large proportion of the cases in which it is employed; and that even in almost all the remainder, though perfect immunity from the contagion of small-pox is not always given, yet, generally, in these a much milder disease, as well as of shorter duration, is produced, than in those who had not been previously vaccinated.

These well established data fully warrant the general adoption of vaccination; but, on the other hand, the increased numbers latterly found susceptible of small-pox after vaccination, more than at former periods, urgently demand constant attention, and fully justify the employment of tests by which the security of those vaccinated may be ascertained; especially since it appears that, when small-pox is rife, many suffer from its baneful effects, who, relying on the security of the antidote employed, had indulged in confident feelings of security.

It is perhaps unnecessary to add, that these tests, whether the inoculation with variolous or vaccine matter, or the exposure of the vaccinated (as on a former occasion I ventured to recommend), should be employed when small-pox is not epidemic, as, at other times, the unprotected are more liable to be affected with the worst and most fatal forms of that disease; and I shall here but briefly advert to the additional evidence which the late epidemic afforded, that the fluids in small-pox are materially engaged,—this opinion, and the practical suggestions which it supplies, being so fully set forth in Dr Walker's inquiry into the small-pox which I have already quoted.

The objects proposed in this letter are offered as the only apology I can plead for addressing it to you, or for occupying so much of your time on a subject with which you are so conversant; but, on the other hand, reminded of the limits to which it should be confined, these will, I trust, excuse me to those who may consider the details I have entered upon not sufficiently extended. I have the honour to be, dear Sir, very respectfully, your obedient servant,

Wm. Stoker, M. D.

To Dr Thomson, M. D.
&c. &c. &c.

Edinburgh, 5, *George Street*,
November 15*th*, 1823.

Dear Sir,

I avail myself of the opportunity which Dr Stoker's communication affords, of stating, that since the publication of my "Historical Sketch of Small-Pox," in January 1822, scarcely a week has elapsed in which I have not seen this disease, or been informed by my medical brethren of its existence, in some quarter or other of Edinburgh. It has continued to attack persons who had previously passed through small-pox, whether natural or inoculated; and those who had been satisfactorily vaccinated, as well as persons who had not gone through either the small-pox or the cow-pox. The attacks of small-pox have been very malignant and fatal in a large proportion of the unprotected; severe in several of those who had previously passed through small-pox; and severe sometimes also, though generally mild, in those who had gone through the process of vaccination.

The general results, therefore, of my later observations and inquiries, have appeared to me to confirm the validity of the opinions which I expressed to you in a letter inserted in the 56th Number of your Journal, with regard to the incalculable advantages to be derived from the practice of vaccination—the frequent occurrence of small-pox for a second or even a third time in the same individual—and the identity of all the kinds of varioloid eruptions described by practical authors under the names of Genuine and Spurious Small-pox.

I beg leave to add, that after an attentive consideration of the *Test Pock* of Mr Bryce, I have been led to believe, that it bears the same relation to the primary cow-pox, which secondary small-pox bear to primary small-pox; and that if medical men, previously to the introduction of vaccination, gave the name of *Varicella* to varioloid eruptions, many of which we have reason to believe were cases of secondary small-pox, we may now, I conceive, with equal propriety, give the name of *Vaccinella* to secondary cow-pox. Cow-pox modify cow-pox, as small-pox modify small-pox; and these diseases produce each a diminutive or spurious pock, in being reciprocally modified by one another. These analogies between small-pox and cow-pox are as curious in a speculative, as they are important in a practical point of view. I remain, dear Sir, very faithfully yours,

To Dr Duncan Junior. JOHN THOMSON, M. D.

XII.

A Letter to John Abernethy, Esq. on the Subject of Hernia. By EDWARD GEOGHEGAN, Surgeon to the Dublin General Dispensary.

SIR,

I HAVE been always of opinion that my professional brethren were in error as to the nature and treatment of Hernia; and believing that former attempts of mine in print, towards its further elucidation, have failed, I wish to proceed with the explanation, and to engage your attention particularly, whose powers of discrimination and candour I so highly value.

When a part that had been contained in the abdomen has protruded, it is easily returned in general, and is to be retained by bandage. When inflammation occurs, and the part is enlarged far beyond the size of the opening, the case is altered entirely, and requires to be treated accordingly; returning it now ought not to be thought of. All the authorities insist that its immediate return, as before the altered state, is the *sine qua non* of cure, and they practise thus:—One hand pushes the hernia at its base up towards the opening; the fingers of the other press at the apex, from side to side, and try, as they say, to get up a small portion, and the rest will follow into the abdomen. At one time the body is recumbent and relaxed, as relaxation is of great moment; at another it is stretched, placed perpendicularly as if on the head, jolted and distorted to disengage the strangulated part. *What a mode of relaxing!* Some conceive that fæcal matter may occasionally be the cause, and try to remove it by pressing with finger and thumb close to the opening, to empty that portion. These manual efforts are conducted with great force by some; and many hands are, in most cases, employed in succession, until stopped by fatigue. Celebrated men say, the hernia is girt as if bound by a cord, and that it is to be forced back into the abdomen, as a first step,—and are very exact that the direction given to the force shall point to the spot from which it descended, clearly that the protruded part may act as a wedge, with due effect, in fulfilment of their curative indication. I object, *in limine*, to the *indication*, as not appropriate. The hernia, in general, had become gradually accustomed to the situation, and performed its functions, free from distress, for a length of time—nay, seemed far more inclined to this situation than to the inside, never remaining within unless held there by bandage. Inflammation, tumefaction, excessive pain, and violent commotion of the system, supervene: it cannot be remedial of these surely to attempt removing the part out of the site to which it was habituated, into the belly, where it might not have been a long time before. Were a limb displaced from its socket, a length of time out, and retained in only by the aid of bandage, to become violently inflamed whilst out, would the indication of cure be, to force it back to the place from which it had been formerly displaced? The principle is the same in both cases. Small *herniæ* have been replaced by force, yet the strangulation was not removed, and death followed. Here the indication was accomplished as advised; and, instead of effecting a cure, prevented it, by placing the gut out of the reach of operation, the *dernier resort*. These reasons satisfy me that the indication laid down is inapplicable, and they are farther strengthened on examining the *modus agendi*. A deep view of the part discloses a fold of intestine, presenting two tubes enlarged by inflammation. The narrow space (containing also the spermatic chord) and rigidity of the surrounding tendons hinder the inflamed coats from expanding: hence they coalesce, a state that closes the tube so as to confine the flatus, and perhaps fæcal matter, the pungency of which adds not a little to the irritation. The circulation is also more or less interrupted. This highly sensible, inflamed and inflated tumour, being pressed from the bottom, the flatus, and also fluids of its coats, must accumulate above, and still farther enlarge there, so as to prevent, instead of promote, the replacement. The working with fingers close to the tendon is rendered useless by the intervening integuments and tense sac opposing their command of the gut; besides, it is girt (they say) as by a cord,—an insuperable difficulty surely (of their own showing) to it, the girded gut, being moved in any way by either of these manœuvres. The effort to remove the contents with finger and thumb, from the portion next the ring, is advised only after the other methods have failed. This is puzzling indeed. The tumefaction is occasioned by flatus, which is removed as by explosion, not *guttatim;* if uninterrupted, the instant it escapes, distension ceases, and further efforts are not required. Again, the plan of replacing the hernia, by wedging with it, forbids emptying, inasmuch as flaccidity unfits it for the purpose: not a particle of air should escape, the better to preserve its firmness. All this time, violent irritation is applied to highly irritable parts, exasperating to a dangerous extent. The impelled hernia being some inches in circumference, and the opening into the abdomen rarely exceeding two inches, it is physically impossible that the former pass through the latter, unless one is widened, or the other rendered small; also, the force imparted to a piece of intestine could never effect the intention against such rigid parts; and a reduced size can only take place by removing the flatus, which being done, all force is lost. The impropriety of the inverted position of the body, as if on the head, tossing it about, together with pushing, kneading, and otherwise bruising the bowels, strikingly illustrates the prevailing error. This representation, as to indication and practice, is borne out by tangible facts, and is undeniable. Were the manual efforts merely unavailing and hurtless, I would decline commenting on them; but instead of their being indicated, they are contraindicated by every sound principle, and the execution carries destruction instead of safety.

Whoever looks minutely into the disease will find, that its *proximate* cause is a closed state of the intestine, and *proxima causa tollenda* is a settled principle. Irritation having produced the mischief, is strictly to be avoided. Handling and jolting, in the first instance, are most hurtful. There is no indication whatever for replacing the hernia in its former birth. The treatment I advise is constant application of cold water or ice locally, bleeding largely from the arm until the pulse sink—to be repeated, should they rise, every half hour for a couple of hours. These means will lessen the local sensibility and general commotion. Then, but not till then, the hands may be safely applied about the middle of the tumour, to compress gently, with a view of expelling the flatus, which, rushing through, opens the intestinal tube, and instantly the strangulation ceases.

The hand, once applied, ought not to be shifted, because reiterated impulses irritate. If this attempt fails, cold is to be renewed, to which the body may be exposed by opening windows. Strict composure, and a relaxed passive position, agreeable to the patient's feelings, are to be enjoined. We know that a gugling noise always accompanies the removal of strangulation: it is occasioned by flatus separating the coalesced coats of the intestine. This practical fact is of the utmost moment, and at once marks the true nature of the disease and remedy. It is decisive of the soundness of the principle I maintain, namely, that strangulation is remediable solely by rendering the gut *pervious*, independent of replacing it; and that, were its replacement practicable, it is improper until the *pervious* state has been effected. When we break wind backwards, a painful sensation precedes, manifestly occasioned by the confinement of pungent flatus, which, escaping with a gugling noise, terminates the uneasiness. Were the expulsion impeded, distress nearly similar to what attends strangulated hernia would ensue, and be relieved only by extricating the flatus. Confinement of flatus in the sigmoid flexure of the colon, and eructations from the stomach, evince the same thing, yet there are no fasciæ or stricture.

In cases of failure of the plan proposed, operation is to be resorted to, not for the purpose of returning the gut, as recommended, but of allowing it to expand itself, which a very small incision of the tendon will effect; then slight compression will expel the flatus. Instead of returning, it ought to be left outside whilst tumefied, as this state shows the tube still impervious. It may be said, that the situation inside the abdomen is favourable to restoring a pervious state; but as there are cases of failure and death, and there is risk, and great force against the sharp border now wounded must be exerted, it ought not to be replaced, if emptying were practicable. After the tumefaction has been removed, the intestine is situated precisely as before; and if it does not adhere, may now be replaced and bandaged to protect it hereafter; but in some cases, the tendency to lie outside is so great, that external aid to keep it inside is unavailing; furnishing still stronger objections to hastily replacing. It is observed, that when a portion of the bowels bursts out at a wound, immediate return of it is necessary certainly; but the cases are dissimilar. Here it is outside the integuments: there is no thickened sac, or covering of any kind—no gradual accommodation of the protruded part to the situation, or lying there at ease, performing its functions free from complaint; and it is exposed to external injury. Hernia is a protrusion under different circumstances, requiring treatment of another kind. I am favourable to early operation, because it affords a good chance that the intestine is uninjured, the inflammation not extended far, and an agglutination of the villous coat not established: above all, it prevents a continuance of mischievous handling, which latter, I am persuaded, often prevents success in any way. When Desault forbade the taxis altogether, he must have been strongly impressed with a sense of the danger attending it. The main error consists in not observing the distinction between the original disease, which is to be treated by replacement, and the new adventitious affection, which has altered the condition of the parts, so as to present other views and different treatment. I have excluded the consideration of the fasciæ, which have engaged so much attention, I think unprofitably. They are not altered, and are merely incidentally in the way of an enlarging tumour, like the arch of a bridge, accidentally choked up. It is underneath we are to direct our views to diminish the bulk there, in the first instance. When operating, wherever the enlarged tumour is most resisted, there the knife should divide. The representation that the intestine is bound as by a cord, is a palpable error, as it contemplates activity in the tendons, *narrowing* themselves in an extreme degree; whereas, on the contrary, the existence of hernia proves that they are *wider* than natural. The surgeon who has such a view is not warranted in the use of any means, except to lay open the parts, and cut the cording in the first instance. What are we to think here of the practicability of immediate replacement? Having discussed this subject in detail in former publications, I limit my present observations to such points as I think particularly require elucidation, and are of deep interest. Hernia is usually considered incurable, and abandoned to bandaging, for the mere purpose of retaining it. A few cases have fallen under my observation of the disease disappearing on the constant application, night and day, of a truss for a length of time.

I am, Sir, with high respect and regard,
Your obedient servant,
EDWARD GEOGHEGAN.

Dublin, August 12th, 1823.

P. S.—When the hernia consists of omentum and intestine, emptying the latter will abate the symptoms, although no part shall be replaced.

PART II.

CRITICAL ANALYSIS.

I.

1. *Fragmente über die Bildung* (Figure, Configuration) *des kleinen Gehirns im Menschen.* Vom Professor REIL. Archiven für die Physiologie. 8ter Band.

Fragment on the Configuration of the Small Brain (cerebellum) in Man. By PROFESSOR REIL. From the Archives of Physiology, vol. 8th.

2. *Anatomical and Physiological Commentaries.* By HERBERT MAYO, Surgeon and Lecturer in Anatomy. Number 1. August, 1822. London, Underwood, 1822. pp. 58.

Inquiries respecting the Structure of the human Cerebellum. By Professor REIL. p. 19.

THE present state of anatomical science, in relation to the knowledge either of the configuration or of the ultimate structure of the brain, is very peculiar and not unworthy of notice. On no other organ have anatomists bestowed more pains and attention,—no other has given rise to so many express or monographical treatises,—and in no other have anatomists been more anxious to communicate distinct notions by elaborate descriptions, varied and magnificent views and delineations, and even in some instances by casts or models of its configuration. The history of our science informs us of the successive labours of Vesalius, Aranzi, and Varoli, in the earlier periods of its existence; the subsequent and more accurate researches of our countrymen Willis and Ridley, and the original inquiries of Raymond Vieussens;—the correctness introduced into the subject by Ruysch, Haller, and Tarin, and many accessory contributions by names of inferior celebrity. A more recent age has witnessed the minute descriptions of Malacarne, the sections of Vicq D'Azyr, and the short but accurate histories of Soëmmering and Bichat. It might appear almost inconceivable, that after the investigations of so many eminent persons, there should be reason to complain of any want of knowledge, or difficulty in understanding distinctly the anatomical relations of an organ so industriously and completely examined. Yet there is no part of the human body which the demonstrator so frequently fails to render intelligible, which the student has so much difficulty in understanding, and certainly none on which the existing descriptions, given even by the latest authors, tend so little to communicate just and distinct ideas of figure, constituent parts, and intimate structure. Every anatomical writer, since the days of Vesalius and Eustachius, has indulged in the hope of adding something to its natural history, or of rendering his descriptions and views more perspicuous, by precision of language or minuteness of detail; and it has been discovered, that notwithstanding these elaborate efforts, many omissions occurred, some errors were committed, and not a few misrepresentations had been taught to the anatomical student. If, indeed, we compare the more recent with the more ancient descriptions, it will be found that exceedingly little has been actually added to the sum of anatomical information; and that copying and imitation have, with the single exception of Malacarne, constituted nine-tenths of the cephalographic works, which have appeared from the days of Julius Cæsar Aranzi, and Constantio Varoli, down to the time of Vicq d'Azyr and Soëmmering. We cannot however believe, that this uniformity of description shows that the subject required no further researches, and was unsusceptible of improvement either in the materials, or in the mode of their distribution. Much reflexion and experience have, on the contrary, satisfied us, that these elaborate descriptions were very remote indeed from completing what ought to be termed, the anatomy of the brain; and that they were, at least in the state in which they came from the hands of their industrious authors, the materials only out of which this more important part of anatomical information might be constructed. An examination rather extensive of the works of authors on this subject, has led us to look on most of their descriptions as a rough and irregular mass; a huge accumulation of materials piled together rather than arranged, without regulating principle or methodic order, —and by no means made to bear on the chief object of their collection,—that distinct and palpable knowledge of the organ, which should enable the student easily to comprehend, and readily to recal, the peculiarities of its mechanical figure, and its material organization. It cannot be denied, indeed, that much has been done by the increasing perfection of the art of engraving, and the multiplication of different views of the organ, to rectify the erroneous notions conveyed by the early anatomists, and elucidate the juster conceptions of the moderns; but a slavish adherence to the same path, and a blind adoption of the same line of investigation, have precluded the possibility of enlarged views; and we may be permitted to say, that correct and comprehensive conceptions of the anatomical characters and relations of the brain, seem to be as remote as ever. Will it be credited, for example, that, in the eighteenth century, the anatomists of this country could be divided in opinion as to the reality of the communication between the lateral cavities beneath the vault (fornix), when, two centuries ago, Varolius, and perhaps some before and many after, took care to show, that the surface of which this part is formed, is free or unadherent, and allows a direct passage from the one side to the other? Is it possible, that the false and groundless assertion, of the first pair of nerves arising from the striated bodies, should still be repeated and taught with the same confidence, as if it was an obvious and indubitable fact, in direct contradiction to what Scarpa, Soëmmering, and Bichat have shown to be the case; or that the second pair arise in part from the four eminences (corpora quadrigemina) of the upper surface of the protuberance, should be admitted on the slender evidence hitherto given? Why should anatomical writers and teachers continue to demonstrate the cavity named the fourth ventricle, and fail to see the true light in which this hollow ought to be contemplated?—or to repeat the useless and antiquated demonstration of the common posterior opening (anus) without unfolding at the same time those just and natural views of the cerebral surfaces to which such a demonstration ought to lead? There can be little doubt that, where such misconceptions prevail, or become the source of controversy, something is still wanting to complete the true system of cerebral anatomy.

The previous considerations, which have been long familiar to us, and in which it is not unlikely that we have been anticipated by many of our readers, have led us to direct our thoughts to the peculiar circumstances connected with the present state of cerebral anatomy, and to reflect on the probable and certain difficulties with which the subject is embarrassed, and the nature of those causes, the influence of which might be conceived to retard or promote this department of anatomical science. We are by no means assured, that we have sagacity enough to recognise all the minute circumstances to which the present confused and uninteresting state of cephalographic anatomy is to be ascribed, or to estimate precisely the force with which each operates in producing its effect; nor are we certain that this inquiry could be in itself either profitable or interesting. The same objection however, does not apply to it when viewed in connexion with the means of improving and amending the state of the science; and we have reason to think, that an exposition of those principles, upon which the correct and rational knowledge of the brain may be founded, will be useful in showing the defects of the present state of information, and in suggesting effectual means for their removal. This purpose, we conceive, will be best attained by bestowing, 1*st*, A very brief attention on the situation in which the anatomical knowledge of the brain has been placed since the revival of literature, till very nearly the beginning of the present century; and, 2*dly*, By showing in what manner the subject may be elucidated, by the adoption of views and principles, which we conceive to be more true to nature, and more consonant to the object of the anatomist, and the principles of human reason.

In science, as in most other things which are subject to the influence of human passion, or those fortuitous events which our ignorance alone compels us to call accidental, nothing is ever produced by the operation of a single cause, or any single order of causes. Numerous complicated circumstances are generally concerned in affecting the result or success of every human pursuit; and it may happen, that either by their concurrent action, or by modifying, or by counteracting each other, the influence of one order of causes will be prevented or neutralized—that of another will be lost,—while the general result will be so disguised and masked, that its connexion with the causing agents shall be with difficulty recognised. This confusion of operation forms a considerable obstacle to the detection of the true circumstances and the individual effect that each may be expected to exercise; and we are compelled to refer them to such general heads as shall most conveniently show their mutual connexion and influence. In the department of anatomical science, to which it is our intention to advert for the present, there are many circumstances which must have been concerned in its advancement or retardation, in common with other divisions of anatomy; but there are also some, the influence of which has been particularly directed on this division of the science. It is more especially the latter which will require our attention; and, if we advert to any one of the former, our reason shall be, that, though its operation may be common to all, its effects will be more conspicuous in modifying the condition of human science as to the knowledge of the brain. The causes to which we allude, may be conveniently referred to the following heads:—1. An extreme or idolatrous veneration for antiquity, and the admission of principles or doctrines which examination would have shown to be hypothetical, not necessarily connected with the subject, or entirely erroneous; 2. Superficial or partial modes of demonstration, by which the relation of the component parts to each other, and to the whole, was either overlooked, or neglected to be taught; 3. The neglect of distinguishing those facts which show the configuration of the brain and its parts, from those facts which illustrate its intimate structure and organization.

It might be presumed, that the extravagant veneration for antiquity, which has been so often, and, perhaps unjustly, charged with unfavourable effects on modern science, should not be allowed to operate on the progress of an art so entirely beholden to the liberal ideas and zealous efforts of the Moderns, as Anatomy. Yet it is not less certain, that many of the defects with which the anatomical history of the brain is embarrassed, are to be attributed to the direct influence of this cause. The auspices under which a subject is at first introduced to the attention of men, not unfrequently continue to modify its subsequent fate; and a short glance at the circumstances under which the anatomy of the brain was pursued or unfolded at the time, named the revival of literature, will show how much its progress was either retarded, or encumbered with useless and injurious impediments.

The close of the Eastern dynasty of the Roman empire exercised a remarkable influence on the state of learning during the middle ages. In the eighth and ninth centuries especially of the Christian era, a great decline in taste, literature and science, began to manifest itself. The sectarian spirit which had taken possession of the Christians of the East, involved them in subtle disputes on abstract subjects,—always unfavourable to true science. The sanguinary persecutions to which these controversies frequently gave birth, tended very powerfully to extinguish the declining spirit of learning among the Alexandrian and Byzantine Greeks of the empire; and, during the course of nearly five centuries, two periods only can be recognised in which any efforts were made to resist the gradual but sure approaches of barbarism and ignorance. The reign of Constantine VII., commonly called the Porphyrogenite, in the beginning of the tenth century, is recorded as favourable to literature and learned men; but the habits of this weak and inactive prince were too languid to make a steady and persevering effort at such an inauspicious period. The termination of the eleventh century (1080) is described to have been more fortunate in the reign of Alexius I., whose life and character are embalmed in the pious recollections of his daughter. But it was the brilliancy of a moment, ready to be quenched by the first adverse breath; and the extinction of the Comnenian dynasty, which occupied the space of a century from the accession of Alexius to the death of Isaac Angelus,—the last of the line,—may be regarded as the termination of science, letters, and civilization in the East. During this time, the luxurious vices and internal disturbances of her own subjects conspired, with the successive and repeated inroads of foreign nations, to weaken and destroy the Eastern empire; and the Greek Christians, who had been always distinguished for their superiority in intellectual attainments, were beginning to lose, with their manly and dignified virtues, their taste for philosophy and their love of the liberal arts. The knowledge recorded in the pages of the ancient physicians began to be lost or perverted, and every science connected with the healing art to be sunk in the most utter degradation. The spirit of learning, however, is seldom long stationary; and it has generally happened, that when the illumination of the human intellect is extinguished in one country, it is kindled in another, and kept alive by different hands. While the temple of science was decaying on the shores of the Hellespont, and Europe was relapsing into barbarism and superstition, scientific institutions were rising on the banks of the Tigris and Euphrates; and the instruction of mankind was destined once more to issue from the centre of Asia. The establishment of a university and hospitals at Bagdad quickly concentrated the general and medical knowledge diffused through the East; and the subsequent formation of academies at Cufa and Bassora, and of a medical school at Damascus, not only furnished the Arabians with the means of becoming themselves learned, but contributed to give them literary celebrity, and to qualify them for instructing others. The military glory of the Saracens, and their reputation in arts, diffused their learning very extensively among the nations of southern Europe; and, during the space of four centuries at least, the systems of education, and the doctrines of science discovered in Europe, were either those of the Greeks orientalized, or were derived directly from the Arabian authors. The Saracenic medicine, thus introduced into Europe, and perpetuated and diffused by numerous universities in Spain, and the celebrated Salernian school in Italy, continued long to be the only system taught, and to guide the principles and pursuits of physicians; and in the beginning of the fourteenth century, which is marked as the dawn of modern anatomy, it was so firmly established in the confidence of men, that no human power was adequate to its subversion or removal. We have examined the work of Mondini, the reputed founder of human anatomy, and find examples of his Arabesque principles at every page; and the Commentaries, written on this early manual by James Berenger of Carpi, fully two centuries after the time of his author, show clearly that the Saracen doctrines, and the Arabian authorities, still kept possession of the medical schools. Yet this was considerably posterior to the flight of the Byzantine teachers into Italy; and even Petrarch, Dante and Boccacio, had lived to improve the taste of Europe by their example, and by their love, and revival of ancient literature.

The introduction of the knowledge of the Greek writers into Italy, at length began to effect a manifest change on the taste and pursuits of the day. The study of the ancient authors of Greece and Rome began gradually to supersede the unnatural manner and the mystical insignificance of the Arabians; and while a purer and more refined taste became prevalent among persons devoted to literary pursuits, the true value of their Saracenic instructors was more justly appreciated. The admiration with which the ancient historians were perused, was transferred without scruple to the writers on medicine; and the respect and deference with which their elegance and dignity were justly contemplated, were most improperly bestowed on their scientific information. The reputation of being a Greek, and of writing commentaries on Hippocrates, secured to Galen the distinction of being received as the oracular authority of anatomy; and when Jacques Dubois, better known under the Romanized appellation of Jacobus Silvius, taught this art in the University of Paris, we are informed that his instructions consisted chiefly of commentaries on the writings ascribed to the Pergamenian anatomist. It is indeed certain, from what is known of the history of John Fernel, Gonthier of Andernach, and Charles Etienne, the contemporaries of this celebrated scholar, that the reverence and respect in which the anatomy of Galen was held, long operated as a bar against the improvement of the science; and the persecution to which the immortal Vesalius was exposed, for presuming to support the evidence derived from human dissection against the authority of the Greek, shows clearly how much the undue respect of the ancients could operate in checking the growth of truth and science. To us, the most wonderful part of this period of anatomical history appears to be the very inconsiderable deviation from the doctrines of Galen which Vesalius attempted; and certainly we are induced not to ask why he did so much, but why he did not do a great deal more. For although he resisted most strenuously the grosser and more palpable absurdities of Galenian cephalography, and would not admit as real, what his dissections showed Galen must have imagined, still his general plan of demonstrating the organ, and his compliance with the custom of the day, in telling what Erasistratus, or Herophilus, or Galen, or Andreas, or Marinus had taught or had thought, evidently confined his views, and embarrassed his principles. He seems to have been much too solicitous to describe only those parts of the organ which had been previously mentioned by the Alexandrian school, and overlooked those objects which did not correspond with the ancient descriptions.

With all these objections, however, the Vesalian account of the brain was a great achievement in anatomy; and had his successors done as much in correcting errors still left, as he did in getting rid of those of the Galenian system, we cannot doubt that our knowledge of the organ would have been in a much more perfect condition than it is at the present day. The prejudices, however, which prevailed against the Flemish anatomist, and the powerful influence of his implacable rival Dubois, contributed to strengthen the authority of the ancients, and to burden the science of anatomy with many injurious impediments. To this we owe the revival and propagation of the absurd notions of the resemblance of the brain to marrow, and the extravagant fiction of the generation of the animal spirits in the cerebral cavities. The lower surface of the organ, the most important of all, was neglected entirely, till the sagacity of Varoli showed the important and original information which its examination was fitted to communicate. But even the value of this novelty was overlooked; and demonstrators and authors continued to repeat the same assertions, in the same useless and uninteresting arrangement, until the works of Willis and Vieussens attempted to introduce some beneficial changes. The former proposed a new method of demonstration, and the latter called the attention of anatomists to the intimate structure of the organ. Although, however, they made some discoveries, the impression was too transient, or the world too indifferent, to be much interested in them; and while these individual instances were labouring to correct error or teach truth, the Professors of the European Universities were delivering the anatomy of the brain in nearly the same style in which it had been taught by Vesalius. The same spirit of examination continued; and though the cause had gradually disappeared from the eyes of modern anatomists, its effects still remained in full force.

This obstinate attachment to the original Galenian or Vesalian anatomy, has encumbered the nomenclature of the subject with the most strange and unmeaning terms. We can conceive nothing more absurd and less significant than such epithets as the *callous body*, *the lyre*, *the sea-horse and his foot*, with several denominations not less fanciful; and it has ever appeared to us the grossest pedantry, and the most vile remnant of literary affectation, to retain these names, invested in all the pomp of ancient phraseology, when they are rarely of any other use than to expose the ignorance of those who use them. Yet they were not equally objectionable in the writings of the Alexandrian anatomists. The epithets applied by them were, in most instances, significant of some familiar object to which the part thus named was believed to bear a resemblance. The smooth or polished body (σωμα τιλλοειδες, corpus laeve) received its denomination to distinguish it from those surfaces, which, if cut with a sharp instrument, might thus be confounded with it. This, however, was absurdly enough rendered *corpus callosum*; and the name has continued in use till the present day. The vault or ceiling (ψαλις, ψαλιδιον, σωμα ψαλιδοειδες, καμαριον), owed its name to its similarity in curved figure and disposition to the roof or ceiling of some of the ancient temples. This term has been rendered in the Latin with sufficient justice *fornix*; and both the French and Italian anatomists have prevented it from being misunderstood, by the literal translation of *voute* and *volta*; but the English authors, in adopting the learned term *fornix*, have given rise to the introduction of others much less significant, and especially the erroneous and misapplied term of *lyre*. Other terms have become improper by their reference to speculative notions. The little cone (κωναριον) is a much more rational name than the hypothetical one of pineal gland. Had these names been translated into the modern languages, with a reference to the signification in which they were used, they would have been much more intelligible and rational epithets than those with which the descriptive anatomy of the brain has been hitherto bedecked. If this was either impracticable or injudicious, new names ought then to have been applied in the respective modern languages, which would have been understood by all, and would have taken from the subject an air of mystery which is exceedingly unpropitious to true science.

2. A direct effect of attachment to the ancient anatomists, is the method of exposition prevalent in the schools and the works of anatomists, and the substitution of mere demonstration for anatomical description. We have long looked on this method of exposition as founded on the most erroneous and irrational principles. It is well known, that the one originally employed by the Alexandrian anatomists was revived by Vesalius, and

adopted by most of the subsequent anatomical writers, monographical or systematic, down to Vicq D'Azyr, Fife, Bell, the London Dissector, and indeed almost all the works usually recommended or employed to guide the student. This method, which consists in beginning with the upper surface of the organ, cutting across the hemispheres, so as to expose the cavities and the objects which they contain, and then proceeding by successive sections to the base or lower part of the brain, is liable to many disadvantages. It affords but a partial, imperfect, and erroneous view of the configuration of the organ; it prevents the dissector from perceiving the relative situation and connexion of its component parts; and communicates a very inadequate and erroneous conception of the important objects which compose the lower surface of the brain. It fails to give a correct and natural view of the cerebral surfaces, since it overlooks their communication, and represents the ventricles as cavities quite distinct from, and unconnected with, the other surfaces of the brain. Lastly, the Vesalian mode of exposition is objectionable, because it confounds the facts relating to the configuration, with those which illustrate the intimate structure of the organ, and thus furnishes an imperfect and partial view of both.

These objections seem to have been early understood by Constantio Varoli, who, in 1573, in his Epistle on the optic nerves, suggested the demonstration from the basis or inferior surface of the organ. This mode of exposition, which was calculated to show some new and interesting points in cerebral anatomy, and might have led to more just and correct ideas on the configuration of the organ, was, however, adopted by few, and never became general. It was known and practised by Thomas Bartholin, Adrian Spigelius, and Willis; but it seems to have sunk gradually into neglect and oblivion, till a recent attempt to revive it was made by Dr Gall of Vienna. The circumstances, however, under which this physician and his coadjutor brought it before to the anatomical world, were extremely unfavourable to its general adoption. Though evidently a very useful mode of demonstration, it must at once occur that it exhibits a view too partial and limited of the subject; and, though well suited when united with other methods to unfold the cerebral anatomy, it is alone quite insufficient to communicate a just view of the subject intended.

The mixed method of exposition proposed by Francis de le Boe of Leyden, and recorded by the grateful diligence of his pupil Thomas Bartholin, seems never to have been very generally known or practised. It has few advantages which are peculiar to it; and the combination of the old Vesalian mode and that of Varoli, would accomplish almost every purpose for which it has been suggested. But neither the adoption of this, nor of any other mode of exposition exclusively, could have improved materially the knowledge or anatomical history of the brain; and we believe it might be shown without much difficulty, that this result must proceed from a more judicious mode of examining, observing, and describing, rather than from any peculiar plan of exposition.

The great error into which, we conceive, all cerebral anatomists have been led, consists in the substitution of modes of exposition or demonstration, for the true descriptive anatomy of the organ. They have believed their task to be finished and their object accomplished, when they have divided the membranes, cut into the cavities, enumerated the objects found, and made a few sections with some vague observations on the *cortical* and *medullary* parts of the organ. This might have been exceedingly meritorious two centuries ago, when the great purpose of mankind was to have their curiosity excited, not gratified, to admire not to be instructed; but we trust that the sound sense of the present age will not be satisfied with such slender and unsubstantial triumph over the march of ignorance. A little reflection will show that it is neither exposition alone, nor dissection, nor even demonstration, with which the student of cerebral anatomy ought to be satisfied. These, indeed, are the means by which he is to acquire his knowledge of the figure and structure of the organ; but they ought never to be viewed as the final object of his researches. They will furnish him with the facts, with the materials, and with the various elements from which he may form correct views of the organ; and, by combining these means with attentive and repeated examination, he will be enabled to frame just notions of the relative anatomy of its various parts,—a subject which, in relation to this organ, has been scarcely understood, or at least not sufficiently investigated.

The same reasons, which prove the absurdity of substituting the means for the end in the examination of this subject, are equally applicable in showing the inutility of pursuing any single mode of exposition. It is essentially absurd to believe that any one mode, whatever advantages it may possess over the others, is sufficient to enable either teacher to show, or student to understand, the anatomical peculiarities of an organ so complicated as the brain. It argues ignorance at once of the principles on which Nature seems to have constructed the animal body, and of the means by which its construction may be unfolded, An exclusive adherence to any one method will be found to be incompatible with just and natural views of the organ; and to exhibit a correct state of cerebral anatomy, it is requisite to adopt several modes of exposition, and to vary or modify these according to the parts to be described, or the relations to be unfolded. The rational principles of descriptive anatomy require the organ to be viewed in different modes, to mark correctly those prominent and conspicuous characters by which its general configuration is determined, and to distinguish accurately those peculiarities by which each part of it is made to differ from the others. It is not the situation, position, or the obvious and common division of the organ to which the attention of the anatomist will be directed; he must examine the relation of its individual parts; when he finds it presents certain cavities, he must observe their direction, extent and configuration, and their relation to the exterior regions of the organ; he will endeavour to discover their connexion with the exterior region, and to determine the nature, extent, and peculiarities of this communication, if it exist. This investigation ought not, as is too commonly done, to be interrupted by any extraneous or desultory observations on the parts exposed in the necessary sections; all this should be postponed till the intimate structure of the organ become the subject of research. The examination of the surfaces, and the objects which they present, ought to be continued from beginning to end, without interruption; and it will be found that they are quite sufficient to occupy the undivided attention of the dissector or student, who is desirous to understand them, and impress them on his remembrance.

Justice to the memory of an ingenious foreigner requires that we should here make a slight exception in favour of the anatomical history of the brain, delivered in the 3d volume of the Descriptive Anatomy of Bichat. He is the first anatomist who, so far as our remembrance will bear, perceived the truth of the principles which we have now attempted to unfold. A transitory glance however, seems to be all that he directed to them; and the ordinary procedures seem to have encompassed him so closely, that he found it impossible to shake himself quite free of them. He recognised the difference between exposition or demonstration, and anatomical description; but in his effort to show that he was conscious of this distinction, and to perform the latter task in the spirit in which he had conceived it, he has allowed himself to be misled by the forms and arrangement of the ordinary mode of demonstration. By a very precise but mechanical division, he examines successively the exterior conformation, the interior conformation, and the organization; and he describes the objects of the two former as they occur either on the mesial plane, or on either side of this imaginary line, in the brain, cerebellum, and cerebral end of the spinal chord. This arrangement, which was evidently dictated by more enlarged views than any hitherto given of the brain, was artificial without being useful, and exact without being scientifically correct. The mode in which he has examined its individual parts, shows too evidently how much he was influenced by the incorrect notions propagated by the Vesalian demonstration; and he has overlooked or denied the important fact demonstrated by Vieussens, that the terms *callous body* and *vault*, were applied to opposite surfaces of the same object.

Bichat has been since imitated by an eminent and lamented anatomical teacher of this city, and in his own country by Cloquet. The description of the brain, given in the first volume of the Anatomy of Dr Gordon, has the extraordinary merit of being the most correct and complete yet published; but it is unhappily arranged on the same principles which form the chief objection to that of the French anatomist. Of that of M. Cloquet, it is scarcely requisite to say any thing peculiar; it is very short, as must be every description in a systematic work; but it is the most precise that we have yet seen, and is infinitely superior to any thing that has yet appeared as a guide in demonstration since the days of Soemmering and Bichat.

3. We have been led to anticipate in some measure the consideration of the third general head, to which we referred the obstacles to correct encephalography. We allude to the practice to which the majority of writers and dissectors have adhered, of confounding the facts pertaining to the configuration of the organ, with those which pertain to its organization, or to the exposition of its intimate structure. The distinction between the anatomy of configuration and that of structure, was early known; and we have proofs of it in the writings ascribed to Galen, and others of the ancient anatomists, and more evident examples in the works of the modern founders of anatomy. We find James Berenger, the *naif* commentator of the Manual of Mondini, adducing at every chapter facts which illustrate the intimate structure of the organs described; and Vesalius has prefaced each of his books with a considerable dissertation on the elementary composition of those organs, the anatomical peculiarities of which he is about to consider. This useful distinction, however, was either never sufficiently learned, or had been early forgotten; and we shall on some future occasion show, that the subject was either entirely overlooked, or treated in the most desultory and confused manner possible. It is well known,

that, in the ordinary mode of demonstration, when the ventricles, as they are named, are exposed, the attention of the student is directed to the white transverse section of the hemispheres, which has been absurdly enough termed the *oval centre of Vieussens.* In the subsequent progress of the enumeration also, the striated bodies are divided by horizontal and vertical sections, and their interior striated appearance shown. This, however, is in general performed as soon as their configuration has been exhibited; and no attempt whatever is made to unfold exactly the disposition or distribution of the cerebral matter of which these parts are composed, or to show the connexion of this with the contiguous regions of the brain. We need scarcely remind our readers of the vague observations which are usually made on the arborescent disposition of the white and grey matter of the cerebellum, and of the careless and unsatisfactory exposition which is thus given of its organization. It is impossible to conceive any thing less rational or scientific, than this irregular procedure in unfolding the structure of an important and complicated organ. It either prevents the figurate appearance of the inner surface from being carefully examined and distinctly understood, or it exhibits an imperfect and confused idea of the cerebral organization, or it contributes most effectually to both. The desultory information which is thus obtained, is confined almost entirely to one or two spots of the organ; and as no effort is made to connect its parts, it will communicate a very meagre and profitless idea indeed, of the intimate structure of the brain. To prevent the inconveniences of this confused and irregular mode of research, every observation which tends to illustrate the disposition of the cerebral matter, should be reserved, with others of the same nature, for its proper place; and, by comparing and connecting them, should be made mutually to explain each other. The interior substance of the several objects on the inner or figurate surface of the organ, should be divided by numerous parallel and perpendicular sections; the appearances thus exposed should be carefully marked, and their successive gradations traced; the connections of the appearance of these sections, with the appearances of sections of contiguous parts, should be observed, and, by judicious and attentive arrangement of the results thus obtained, the dissector will be enabled to form on the cerebral organization conclusions, which, for accuracy, novelty, and real importance, he will in vain attempt to obtain by other means. As a final and necessary auxiliary in this inquiry, the ultimate arrangement of the cerebral atoms or molecules ought not to be omitted. In the various parts of the recent brain, sometimes by the naked eye, always by the aid of a glass of moderate powers, an arrangement of minute parallel capillary lines may be recognised; in other situations where this cannot be perceived, a particular arrangment of very minute globular bodies is perceptible. It is the duty of the anatomist, whose object is the organization of the brain, to discriminate carefully the situations to which these peculiarities are respectively confined, to observe the modifications, varieties, and combinations which they undergo; and, in short, to examine every fact which can illustrate this intricate and uncertain region of anatomical science. The effects of chemical agents ought not to be omitted; and it will frequently happen, that where parts are obscure or confused in the recent organ, the employment of a suitable agent for a few days will be of the utmost utility in facilitating the discovery of the truth. It is scarcely necessary to mention, that in conducting such inquiries, the erring and hypothetical distinctions of cerebral matter into cortical and medullary ought to be discarded; and that the labours of modern chemistry have shown, that it is vain and nugatory to seek to compare to any known or familiar substance, either in chemical nature, or in physiological character an organic substance, the properties of which we are only beginning to study.

The principles which we have now attempted to unfold, will qualify our readers, we trust, to estimate with some approach to truth, the merit of the descriptions hitherto given, and of the works hitherto published, on encephalographic anatomy. They will show that it is not so much by new methods of exposition, or by discoveries, as by a more rational application of the old, that this department of science may be expected to be improved. They, however, show also, that much yet remains to be done in order to bring the knowledge, or even the inquiry regarding the cerebral organization, into a proper state; and that, however much the brain has been dissected and described, what has been done is either so vague, imaginary and incorrect, or that which is correct, is so little known or studied, that its utility is almost completely lost to the anatomical world.

The last of these circumstances is exemplified with much force in the fate of an inquiry of this kind, to which our duty requires us to give at present a short and passing attention, at the risk of violating somewhat the unity both of our purpose and of our present train of reflections. John Christian Reil, Professor of Anatomy in the University of Halle, instituted, about the years 1795, 1796, 1797, a series of very difficult but interesting researches on the structure of the brain, the results of which he published in successive memoirs in the Archives of

Physiology, conducted by himself and Professor Autenreith;—of the numerous journals published in Germany, by far the most valuable. Professor Reil commenced the publication of his Researches with a description of the cerebellum and its parts, in which, as he avowed, and as we shall afterwards show, he followed closely the Essays of Vincenzio Malacarne. This appeared in the first Number of the 8th volume of the Archives, which corresponds to the years 1807–8. This was succeeded by two Memoirs, containing a first and second series of researches on the structure of the cerebellum, in the 2d and 3d Numbers of the same volume; and a third and last series of researches on the generalities of the same subject, was published in the form of a short Appendix, in the 1st Number of the 9th volume. The same Number contains the fourth series of researches, in which Professor Reil proceeded to unfold the structure and organization of the large brain. This Memoir, which is very long, and contains much minute and accurate description, is by far the most elaborate and valuable of his papers, and communicates an immense quantity of curious and original information on the subject. The third Number of the same volume presents the fifth series of researches, which are in some sense to be viewed as the continuation of the fourth series, in the form of observations on the structure of the cerebral end of the chord, and the limbs of the small brain. The publication of this part of Reil's labours, which took place in 1809, was succeeded by a long interval; and his other engagements seem to have deferred his further illustration of the subject till the year 1812, when we find him, in the 11th volume, resuming his inquiries in a dissertation on the anterior commissure of the large brain, in which he describes minutely its relations and its connexion with the cerebral matter of the hemispheres. This Memoir, which forms the sixth series of Reil's researches, is immediately followed, in the same Number, by a seventh series, on the partition-wall (septum lucidum), its cavity, the twain-band (Die Zwillingsbinde, fornix), and the general cavities of the organ. An Appendix, in the 3d Number of the same volume, forms the eighth and last series of researches, in which Professor Reil delivers much new and original information on the beam (Der Balken, corpus callosum), and its connexion with the limbs of the brain, and on the organization of these various parts.

Such is a general outline of the researches which Professor Reil instituted on the cerebral anatomy. We believe they were carried on sometime between the years 1795 and 1812, as we know, from personal and direct testimony, that he was occupied in them at Halle at the former of these periods; and before the termination of the latter, the Memoirs which we have enumerated were published. Yet at this hour, they are neither generally nor extensively known in the different countries of Europe, and in many their existence was never heard of. Bichat was the first who gave any attention to his dissections, and noticed some of the results in his general anatomy of the nervous system; but he acknowledges that the dissections are extremely difficult, and that he had not practised many of them. In this city, though the researches of Reil were not unknown, they were taught publicly by the late Dr Gordon only, in his anatomical lectures. The diffusion, however, of scientific information through such a channel, is necessarily circumscribed; and, without means of more general operation, it cannot be expected that they will become so generally known as they ought. If this can be effected by divesting them of the forbidding disguise of a language not generally understood, the small work which is announced at the beginning of this article will contribute in some slight degree. It contains matter somewhat miscellaneous, and of various merit; but the part to which our readers' attention is now requested, is a translation of Reil's three first Memoirs and the Appendix—containing, therefore, the great part of the German anatomist's researches on the cerebellum. In this, Mr Mayow has performed the duty neither of a close and faithful translator, nor that of a mere editor of the discoveries of his author, but has, in an intermediate and indefinable character, translated and abridged the Memoirs of Reil, while he has not exhibited them in that luminous and distinct arrangement, which we think would have greatly contributed to render his researches more universally known. We believe it would have been more judicious to have either translated the whole of Reil's Memoirs exactly as they stood, and left the anatomical reader to judge of their merit in arrangement, matter, and originality; or, if a discretionary power were to be at all permitted, to have made such changes in arrangement and selection, as would have given the public the full benefit of the researches of the ingenious author, without depriving him of that merit to which original genius, especially when employed in the patient and elaborate investigation of natural objects, is undoubtedly entitled. Had the translator commenced his labour with the memoirs on the brain proper, and then proceeded to those on the cerebellum, without regarding the arrangement of the original, which was suggested by particular views of the learned author, we are disposed to think that he would have produced a work which would have been more readily recommended to the attention of the profession, by arrangement similar to that to which they have been accustomed. It would also have been desirable to illustrate the peculiar terminology and views of Reil by suitable references to other authors, which would have rendered his descriptions more intelligible, and given a greater degree of value and accuracy to the meagre accounts of former inquirers. In the present specimen of abridged translation, so little is this observed, that it is impossible for the uninitiated to recognise a single point of resemblance between the epithets of Professor Reil and those of other anatomists; and it would be incredible, that he was describing an organ about which anatomical authors have so generally written, as the brain. This may be deemed in some respects an advantage; but, as the translator has expressly stated, that he gives the matter of the Memoirs, and has condensed the descriptive details where they are obscure or intricate, we must be allowed to express our opinion, that much would have been added to the clearness and simplicity of the descriptions, by explanatory references to the works of those authors, with whom students and professional readers are most familiar.

The translation itself appears to be executed with some care; but many passages have occurred to our perusal which certainly do not express the exact meaning of the original; and in one or two places we met with positive errors, which we were unable to correct till we turned to the Memoirs of the author. The epigraph of the first Memoir (p. 19) is incorrectly rendered, " On the structure of the human cerebellum. " The original German word (*Bildung*) is never used to signify structure, but uniformly configuration. Its literal meaning is *shaping* or *figuring;* and Reil, who devoted this memoir to the exposition of the exterior figure or shape of the organ and its divisions, expressly entitles it, " Fragment on the Shaping or Configuration of the Small Brain in Man. " * The mind of Reil was too clear, and his thoughts too precise, to confound the configuration with the structure of any organ; and he accordingly, in the second and third Memoirs on the structure of the cerebellum, takes care to employ the word *Bau* (structure), to prevent it from being confounded with the term which he had previously employed to denote the configuration. It is needless to say, that the same distinctions between the words *Bildung* and *Bau*, are observed by Reil in his subsequent Memoirs, and indeed by all the correct German anatomical writers. In the same Memoir, the passage relating to the several lobes into which Reil, after Malacarne, divides the cerebellum, labours under the two-fold defect of being neither literally nor correctly rendered. In the original, the three various lobes are designated, *anterior-upper*, *posterior-upper*, *posterior-under*, without any connecting particle; and this style of expression, though awkward when grammatically viewed, is most scientifically correct, since each thus expresses a single object. Our translator, however, not satisfied with this unsophisticated simplicity of his Teutonic original, by the insertion of a conjunction, actually converts one lobe into two. This mode of expression might have been overlooked as not leading to any irretrievable error, had it occurred only once; but what shall we say of the following sentence?—"Immediately below this, the short and exposed commissure (die kurzen und sichtbaren Querbander), by which the under and posterior lobes are united; below this again the long and hidden commissure (die langen verdeckten Querbander), by which the under and posterior, as well as the slender lobes are joined together." p. 23. Whoever reads this sentence will undoubtedly suppose, that the under and posterior lobes mentioned in the first member are two separate objects, which are thus connected to each other. This, however, is not the fact;—the under and posterior lobes of Mr Mayo are simply the postero-inferior lobes of Professor Reil, each of which being placed on opposite sides of the mesial plane, are here mutually connected, as the translation should have specified. This mistake has evidently given rise to the more serious and awkward one in the following member of the same sentence,—in which a clumsy circumlocution is employed, to veil the true signification. The meaning of the author would have been more truly expressed by translating this concluding passage, "by which the postero-inferior and slender lobes are connected."

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* Fragmente über die Bildung des kleinen Gehirns im Menschen, vom Professor Reil.

In pages 42, 43, in which the first figure of the third engraving is explained, the whole explanation is involved in much obscurity, by not attending to the distinction of the terms *Zweig*, twig or branch, and *Zerastelung*, sprouting, spray; and in the explanatory reference concerning the central lobe, an important error is committed, by saying that it is divided into three branches instead of two, as in the original. These oversights, which, in other circumstances, would be trivial, acquire a serious consequence in a work translated from a language not much read, and especially on a subject of which very few are in a condition to judge. We are unable, however, to spend more of our time in noticing others, as we must now return to the principal subject with which we commenced, and from which we have been compelled to digress, merely in consequence of the connexion which it bears with the researches of Professor Reil. We proceed, therefore, to apply the principles which we have already explained, to show in what manner the anatomy of the brain ought to be treated, and, at the same time, to lay before our readers a short view of those facts which, we believe, have either not been generally known in this country, or, if known, have been neglected.

Let two or three specimens of the organ be removed carefully, and without injury, from the cranium; let the membranes and their vascular connexions with the organ be divided and cautiously detached, so as to preserve the cerebral matter as entire as possible; let one specimen of the organ thus prepared be kept entire and unchanged; from a second let the cerebellum be removed, by a transverse section of the part named the protuberance (pons, tuber annulare) as near the cerebellum as possible; of the third let a section be made removing the upper parts of the hemispheres, on a level with the upper surface of the middle band (mesolobe, corpus callosum) and their posterior ends on a level with the posterior end of the same body; and, if a fourth specimen can be procured, it will be useful to make a vertical section of the brain only on one side of the mesial plane, and by cutting through transversely the cerebral limb of that side. Other sections will occur to be made in the course of the demonstration, to establish particular facts; but with these as preliminary measures, the following inferences will be easily recognised.

The entire organ comprehending the great and small brain, or the brain proper, and the cerebellum, is a body of a peculiar figure, but approaching to that of an oblong spheroid. The division into brain proper and small brain, and of cerebral and cerebellic hemispheres, with the circumstances on which these distinctions are founded, are well known.

The usual division of the brain proper into several lobes is unnatural, adventitious, and useless. It is not founded on any distinct or well-marked boundaries proper to the organ, unless we admit the curvilinear hollow (fossa Sylvii) to be one; it is derived solely, we may be allowed to say, from marks restricted to the confirming osseous case;—and it communicates no information on the natural divisions of the organ. For these reasons it may be justly laid aside.

The brain proper presents two surfaces; one exterior, cranial, or convoluted,—the other interior, central, figurate, or symmetrical. The former of these, which is exterior, is greatly more extensive than the latter, not only in the superficial space which it occupies, but also in the manner of its distribution. The convoluted surface of each hemisphere may be conveniently divided into the following five regions; 1. The commutual or dichotomous; 2. The lateral-superior, or convex; 3. The antero-inferior, or frontal; 4. The medio-inferior, or sphenotemporal; 5. The posterior or cerebellic region of the convoluted surface. The first of these regions of the convoluted surface is easily understood. Plane in its surface, of a shape nearly semicircular, it forms the central boundary of each hemisphere, corresponds to the falciform or dichotomous portion of the hard membrane, * by which it is separated from the similar surface of the opposite hemisphere. Before and behind it extends from the superior to the inferior surface of the brain; but a considerable portion of its middle is interrupted or terminated by the upper surface of the object which we name middle band, (*mesolobe, corpus callosum*), which here lies between the two hemispheres. It is contained between the semicircular and the rectilinear margins. The second region of the convoluted surface is very extensive, and occupies the whole of the anterior, upper, lateral and posterior parts of the hemisphere, from their anterior to their posterior extremity, and from the semicircular margin to a line which extends between these extremities along the lateral borders of the organ. The antero-inferior or frontal, is that region of the convoluted surface which rests on the horizontal part of the frontal and ethmoid bones, and commencing before with a curved outline,—the anterior end of the hemisphere, is bounded behind by the curvilinear hollow, which has been named the pit or fissure of Silvius. It is very slightly uneven, and presents a peculiarity which shall be noticed in its proper place. The medio-inferior or spheno-temporal, is situate immediately behind this region, from which it is separated by the curvilinear hollow. In the ordinary descriptions, this forms what has been irrationally enough named the *middle lobe* of the brain, while the posterior part of the convoluted surface, or that which corresponds to the cerebellum, though distinguished by no evident mark or limit, has been with equal impropriety named the *posterior lobe.* If the whole region be examined from the curvilinear hollow to the posterior tip of the hemisphere, no mark, or line, or boundary, is found on which to establish this popular and much used division; and the whole presents a uniform region of convolutions, which resemble in every respect those found on other parts of this surface. The whole region, therefore, ought in truth to be viewed as a single division of the convoluted surface; but as its posterior part rests not on the cranium, but on the horizontal portion of the hard membrane which covers the small brain, while we discard the erring and forced division of lobes, we think there is no great harm in subdividing the surface into two, the medio-inferior and postero-inferior regions of the convoluted surface, according as they correspond to different containing parts. The first, which near the curvilinear hollow is slightly convex or rounded, is lodged in a considerable cavity of the cranium, formed by the sphenoid and temporal bones, bounded before by the spheno-frontal arch, and behind by the pyramid or petrous portion of the temporal bone. This part of the convoluted surface, which may be also named the spheno-temporal, is one, as we shall find, of very considerable interest and importance, and should be accurately and intimately known by the anatomical student. The posterior division of this region, which is plane, corresponds to the horizontal or cerebellic part of the hard membrane, and, though not to be distinguished by any minute or organic limit, may however, for the sake of more precision, be marked by this adventitious or esoteric character.* We have named it the cerebellic region of the convoluted surface.

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* It is certainly better, if terms must be adopted from ancient authors, to adopt the literal translation of that used by the Alexandrian anatomists *μηνιγξ σκληρη* (meninx dura), than the absurd and hypothetical epithet (dura mater) of the Arabian school.

This view of the great boundaries and chief divisions of the convoluted surface of the brain, should be followed by the description of its peculiarities, or more minute individual characters. These are not numerous; and either our ignorance or the nature of the subject itself, has reduced all that we know to a few general facts. The ordinary appearance of the convoluted surface is well known. It is formed of cerebral matter, of a grey or dirty wax colour, the surface of which is smooth and polished, where it has not been rent by the removal of the membranes and their attachments. The convolutions are found to consist of longitudinal eminences, rounded transversely, running in various directions, and separated from each other by deep furrows. If these be examined when the membranous coverings are removed, they are observed to present many minute orifices, into which the soft membrane (λεπτη μηνιγξ meninx tenuis, *pia mater*) transmits filamentous bodies, most of them minute blood-vessels. They are neither arteries nor veins exclusively, but seem to consist of both. Neither the eminences, nor the hollows or depressions, are uniform in number or distribution; and in no two brains is it possible to trace any similarity in the figure, presence, or direction of these objects. This must be understood of the whole upper, lateral, and posterior part of the convoluted surface, and, in short, all its divisions, unless where it approaches the central or figurate surface. In the latter situation, the round of uniformity which we have noticed disappears, and a number of important and interesting objects are presented to the attention of the observer. The points at which this approach of the two surfaces takes place, are, 1*st*, Along the rectilinear margin of the commutual region, where it is contiguous with the upper surface and the posterior end of the middle or central band; 2*d*, From the last of these situations, on each side over the protuberance and cerebral limbs; 3*d*, From this again by the outer margin of the cerebral limbs, to the curvilinear hollow, and along its course. It is in the last of these situations chiefly that the convoluted surface becomes conspicuously important, and at which it exhibits objects which distinguish these regions from the others. One of these only we shall now notice, as the others will more suitably be considered when we speak of the communication of the two surfaces. The antero-inferior region of the convoluted surface is bounded at its inner or mesial margin, by the great fissure which separates the hemispheres. This inner margin always presents one convolution, which is quite uniform in direction, extent and configuration. It consists of a longitudinal eminence, which extends in the adult brain about 1½ inch from the beginning or posterior end of the notch, towards its anterior extremity. The inner margin of the eminence, which is about four lines broad, forms the side of the fissure, and its outer margin or border is separated from the contiguous part of the convoluted surface, by a furrow or hollow equally uniform in direction and figure with the eminence,—about the same average length (1 inch 5 lines). It is remarkable for lodging the cerebral portion of the first pair or olfacient nerves.

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* Lest the use of these terms may be objectionable by their obscurity, we may observe, that in describing parts of the human body, it is not unfrequently requisite to have recourse not only to marks or characters on the organ described, but also to certain characters belonging to the contiguous parts. The first of these may be named the *organic or innate*, as they belong to the organ; the second, which do not belong to it, should be named *adventitious or esoteric*. This is indispensable in relative anatomy.

The outer surface of the cerebellum, or small brain, differs from that which we have now described, or the brain proper. It cannot be said to be convoluted, for it does not present the tortuous eminences and furrows which constitute the convolutions of this part of the organ; but the cerebral matter of which it consists is disposed in the manner of plates (laminæ) or leaves (folia) parallel to each other, or at least concentric, and separated by parallel or concentric furrows. It is scarcely requisite to say, that this definition is not meant to imply, that the direction of all these objects is the same throughout the whole organ,—but merely that the cerebellic plates of which certain groups consist, observe the same direction;—and that any one or two plates or leaves have several of the contiguous ones parallel or concentric with them, while those of the next group, though disposed differently, observe the same direction in relation to each other. By this peculiarity, the various regions of the laminated or foliated surface of the small brain may be distinguished. The plates of the hemispheres are curvilinear and concentric, and pursue in various regions of the organ certain definite directions; those in the middle between the hemispheres are straight, transverse, and parallel; and at one spot they are oblique and parallel. By means of these invariable characters of the cerebellic plates, the surface of the organ may be conveniently distributed into several divisions.

An acquaintance rather particular with the writings of anatomists, has satisfied us that little or no attention was paid to the cerebellic plates or their peculiarities, before the time of Vincenzio Malacarne, professor of surgery in the city of Acqui, in the duchy of Montferrat. This learned person, who was unquestionably one of the most patient and diligent descriptive anatomists of his time, published in the year 1780 three Treatises on the Anatomy of the Brain; * in the third of which he described with much precision and minuteness, the anatomical characters of the outer or laminated surface of the cerebellum. The minute detail with which he has described this surface, and the extreme precision of which he has proved the subject to be susceptible, are sufficient to cause wonder, if any thing in medicine could give rise to such a feeling, that his labours did not become more generally known, and that succeeding anatomists did not attempt to imitate the industry and accuracy of this scientific Italian. If a conclusion could be formed, from the character of the ordinary works published in this country, and the usual style of treating anatomy, we are compelled to say, that the researches of Malacarne were completely unknown. Not so with the eminent German anatomist, whose Memoirs are now before us. The distinctions and terminology of Reil in the cerebellum, were, with few exceptions indeed, first used by Malacarne, from whose third Treatise the German professor avowedly adopted them without alteration; declaring, that it was impossible for him to follow a more true or intelligent

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† Encefalotomia Nuova Universale di Vincenzo Malacarne, Saluzzese. Torino, 1780.

(geistreich) observer of nature, and who, in the dissection and description of the small brain, had accomplished more than all the anatomists who had preceded him. The matter indeed of the first Memoir of J. Christian Reil, is little else but that of the third Memoir of Vincenzio Malacarne; and as we cannot hope that the limits of our present examination will allow us to enter on the subject of the succeeding Memoirs, we shall, without confining ourselves rigorously to the translation of Mr Mayo, take a short view of the manner in which these anatomists examined the outer surface of the cerebellum.

Commencing with the well-known division of the whole organ into two hemispheres, Malacarne remarked, that if the whole upper surface of the organ be presented to the eye, the outline of each hemispherical surface will be found to describe three-fourths of a circle; and as these circular segments mutually meet towards the mesial plane, where they are respectively adapted to different parts, the mode of union varies according to the figure of these adjoining objects. 1*st*, As the hemispherical border approaches the anterior part of the organ, it is found to be suddenly interrupted, where the cerebellic branches or peduncles (crura cerebelli) are connected with the protuberance, and, pursuing a retrograde direction on each side towards the mesial plane, forms a species of re-entrant curvature. The hollow thus formed, which corresponds to the lower of the four eminences on the upper surface of the protuberance (*corpora quadrigemina*, Die Vierhugel), was named by Malacarne the *semilunar curvature*,—a denomination which was adopted without alteration by Reil. 2*d*, Again, as the hemispherical borders approach the posterior part of the small brain, advancing nearer to the mesial plane, they proceed, by an acute circular turn, almost straight backwards, so as to form, at the posterior edge of the organ, a deep rectangular notch, \/, not unlike the figure of the ancient lyre. This posterior hollow, in which is lodged the cerebellic vertical portion of the hard membrane (falx cerebelli), was named by Malacarne the *common perpendicular fissure* (*incavatura*). The German anatomist, to whose fancy this epithet seems to have been deficient in expression, gave it the name of *the purse-like fissure* (Der beutelförmige Ausschnitt.) Between these two well-marked boundaries, the cerebellic plates of which the hemispheres consist, are united in the middle by a confused interlacing junction (un intreccio confuso ed irregolare di sostanza), to which the Italian anatomist gave the name of suture (*raffe*, raphe) of the cerebellum. We have found, that the careful removal of the soft membrane (*pia mater*) renders this more distinct, and shows the mesial termination of the hemispherical plates. On the surface, a large hollow between the hemispheres, and extending backwards from the semilunar to the purse-like fissure, had been previously called by Haller (*vallecula*), the little valley, Malacarne adopted the corresponding term (*valletta*), in his own language.

The divisions of the cerebellic surface which Malacarne made, and which Reil adopted, were founded entirely on the groups of plates, and the more or less considerable depth of the furrows by which these groups were separated. Groups of plates, separated by the deepest furrows, were named lobes (lobi, M. Lappen, R.); and those separated by furrows of less depth, were named lobules (lobetti, M. Läppchen, R.) In some situations, the lobules presented divisions formed by furrows of less depth, between which the groups of plates were of greater or less size. To such clusters, Malacarne gave the name of laminar leaflets (*foglietti laminosi.*) *

Each hemispherical surface consists of five lobes: 1. The anterior-upper; 2. The posterior-upper; 3. The posterior-lower; 4. The slender, rarely exceeding three lines in breadth; 5. The two-bellied or biventral. The two first belong to the upper or flat hemispherical surface; the three latter to the lower or convex hemispheric surface. Besides these, a sixth lobe may be mentioned as common to the two hemispheres. It is situate on the mesial plane of the upper surface, between the anterior end of the middle line (*raffe*), and the middle or apex of the semilunar fissure. This situation is not an improper reason for the name by which Malacarne has distinguished it,—the central lobe.

In the bottom of the purse-like notch, we find many bundles or clusters of plates, which unite the posterior lobes of the upper and lower surfaces to those of the opposite hemisphere. These Malacarne has named *transverse laminar chords* (*cordoni laminosi traversali*), or commissures of the cerebellum. We shall have occasion to notice the view which Reil has taken of these objects.

The anterior-upper lobe, with four sides and four angles, was therefore named by Malacarne the quadrilateral or four-sided lobe,—a term which was literally and correctly adopted by Reil (vierseitige), but which has been most erroneously rendered by Mr Mayo, *the square lobe.* Every beginner in geometry knows, that what is square is necessarily four-sided; but he likewise learns, that what is four-sided is not necessarily square. This distinction, so true and so obvious, is most necessary in describing parts of the human body,—which was certainly constructed without much regard to strict mathematical principles, or accurate geometrical forms. The four-sided lobe of Malacarne and Reil, bears no resemblance whatever to the square figure, as Mr Mayo must remember; and if he was dissatisfied with the respectable compound of Malacarne, or the more homely quadrisyllable (vierseitige) of Professor Reil, he might have gratified his geometrical propensities without impairing the propriety of the denomination, by likening this lobe to a trapezoid, to the figure of which it makes some distant approach. To abandon, however, these unsafe analogies, we would say, that it is bounded by three curved margins, and one straight one. Of the former, the most anterior forms one-half of the semilunar fissure; while the posterior, which is also the longest, and parallel to this, is a curvilinear or circular tract (the great furrow), extending from the bottom of the purse-like fissure behind, to the anterior outline of the hemisphere before, where it terminated about one inch from the end of the semilunar fissure. This last space between the anterior end of the great furrow, forms the third curved margin of the four-sided lobe; while its straight margin is made by the middle line, which here is common to the two lobes. Malacarne further divides this lobe into five lobules, and describes the limits of each with minuteness and accuracy. As, however, we have not space for this, we must refer to the original.

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* Encefalotomia nuova &c., Articolo i. No. 13.

The posterior-upper lobe,—the second division of the upper hemispherical surface,—may be defined in the following manner. The circular tract or great furrow, which we have already said forms the posterior margin of the four-sided lobe, is its anterior boundary. The hemispherical outline of the upper surface, if traced from before backwards, will be found to coincide about 1½ inch, sometimes more, from the purse-like notch, with a considerable furrow (the horizontal), which turns round at the purse-like notch, to meet the great furrow already mentioned. The curved outline thus continued is the outer boundary of the posterior lobe; and it is easy to perceive, that, in consequence of the direction which this line observes, and its meeting with the great furrow, the lobe is contained between the horizontal and great furrow, or between two curved lines. This lobe is subdivided by Malacarne more minutely than the former. It is found, however, that the number of lobules is not the same in both,—those of the left being most uniformly about eight; while those of the right are more numerous, but, in general, less distinctly marked.

These with the central lobe, the general situation of which has been already noticed, form the several divisions of the upper region of the cerebellic laminated surface. The inferior region presents divisions more numerous, more complicated, and more interesting.

The first of these, the posterior-lower, is contiguous, at its outer or greater boundary, with the posterior-upper lobe, where a small segment of it is seen when the cerebellum is examined from above. Its anterior or inner boundary is marked by a curvilinear furrow of moderate depth, by which it is separated from the slender lobe, and the posterior end of which terminates, not in the purse-shaped notch, but, after a sigmoid turn towards the laminar pyramid, † is insensibly lost among a cluster of transverse plates, which we shall have occasion to notice afterwards.

Immediately anterior to this is found the slender lobe, which is not above three lines broad, and, being contained between two concentric furrows, is not unlike the segment intercepted by the truncated arcs of two small circles of a sphere.

The space of the hemispheric surface within the slender lobe, or between this and the peduncle or arm, is enclosed by three circular lines, two of which, the exterior and interior, correspond to so many furrows, while the third, which is anterior, is formed by the horizontal or marginal furrow. The space thus enclosed is similar to a spherical triangle, and is occupied by a group of plates, which Malacarne denominated the *biventral lobe.* Its contiguity with the cerebral end of the pneumogastric nerve, led Vicq D'Azyr to give it the name of *the lobule of the eighth pair* (lobule du nerf vague); and its situation at the posterior corner of the peduncle, and under that body, is the reason why it was called *sub-peduncular lobule* by Dr Gordon. The arrangement of these is peculiar, since they are neither so exactly concentric as in the other lobes, nor yet is their direction different from each other. The plates at its outer margin are the largest, as they extend the whole length from the marginal furrow to the end of the slender lobe, at which, however, they are contracted to a narrow point. The next set are shorter, and are more contracted or acuminated at their posterior end, where they are contiguous to the almonds or tonsils. The third and last set are the shortest, and are more twisted down, as it were, next the almonds. These contracted or acuminated ends of the cerebellic plates have been named (code) tails by Malacarne.

The disposition which we have now described, renders the posterior corner of the biventral lobe very pointed, and its margin very concave; and between this margin and the parts which occupy the valley, is placed a group of plates, somewhat convex, rounded, and disposed also in a peculiar manner. This group Malacarne named the tonsil, or tonsils (*tonsille*),[†] a term which Reil, regarding it synonymous with *amygdalæ*, rendered literally almonds (Die Mandeln), and which has finally become, in the hands of Mr Mayo, *the almond-like lobules*. The tonsil of Malacarne, the almond of Reil, is *the spinal lobule* of Dr Gordon. If the cerebellum be examined before the head of the spinal chord is cut from the protuberance, the inner margin of the almond will be found contiguous to the oval eminence, and even pressed by it. It is best, however, to examine the almonds after the chord (medulla oblongata) has been removed. It will then be perceived, that each almond is bounded on the outside by a well-marked circular furrow, which separates it from the biventral lobe on the inside, by a free surface directed to the corresponding surface of the opposite almond; and before, by a continuation of this directed outwards toward the biventral lobe. We have already said, that the direction of the constituent plates of this body is peculiar. In general, they observe a direction opposite to that of the biventral plates; so that, if produced, they would cross. Its apex, or most pointed corner, is contiguous to that of the biventral lobe; and altogether, each almond presents the appearance, on a cursory glance, of two similar bodies directed inversely to each other.

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† Encefalotomia, &c. Articolo I. No. 20, ed. Articolo IX. No. 77.

The last body which we shall notice here is the Flock. The description of its situation, as given by Malacarne, is by no means clear; but perusal of the passage, with examination of the parts described, can leave no doubt on the certainty of the object which he has in view. The flock (il fioccho, i fiocchi, Die Flocken) is a minute body, of a shape not easily defined, situate in the angular hollow between the biventral lobe, and the branch or peduncle (crus, gamba, braccia, Die Arme) of the small brain. The lower or free surface of the latter object (the branch) will be found to possess an anterior and posterior margin or corner. The latter is contiguous to the biventral lobe, from which it is separated by a small furrow, out of which the flock seems to issue. The description given by Reil (p. 25.) is short, but good; the concluding observation, however, on their being abortive or embryo lobes, is gratuitous, and perhaps untrue.

Ruysch has represented in their site objects to which he has applied the name of vermiform prominences; a circumstance which we cannot explain, but which is in some measure to be ascribed to the vague manner in which this term has been used.

The valley (vallecula, valletta, Das Thal) or hollow between the two hemispheres, is occupied by a numerous series of plates, which lie transversely, are parallel to each other, and which form a sort of uniting medium of the cerebellic plates of each side. The appearance of this region in the brain of the dog and monkey, the animals which the ancient anatomists chiefly dissected, might furnish them with some reason for applying to it the name of worm, or worm-shaped body (σκωληξ, επιφυσις σκωληκοειδης); but we cannot perceive that the modern anatomists are therefore bound to adopt and apply it to the corresponding part of the human cerebellum, which certainly by no means justifies the appellation. The confusion and inaccuracy on this very subject, which has arisen from this unreasonable determination of imitating the ancients, is a sufficient reason to abandon the term for ever,—since scarcely two anatomists will agree in giving the name of vermiform process to the same part of the organ. With a view to obviate all misconception on this point, Malacarne had recourse to distinctions completely new. Beginning with the back end of the valley at the purse-shaped notch, and proceeding forward, he observed a group of parallel plates, or laminated leaves, to which he applied the name of *pyramid.* This body which is bounded behind by the purse-shaped notch, and before by another cluster of plates called by Malacarne the *uvula*, consists of twenty parallel plates, of which six, he observed, were very short; and a triangular termination in the longer, formed the summit of the body. The uvula, which is anterior, he found to consist of twelve laminated leaves, to have six lines of longitudinal extent, and four of breadth.

[†] Encefalotomia, Articolo IX.

Vicq D'Azyr, who avows that he could not understand the distinctions of the Italian anatomist, made one for himself. He looked on the whole of the transverse plates between the two hemispheres as a great vermiform process, which he subdivided into four regions, the anterior, superior, posterior, and inferior. The first of these is the central lobe of Malacarne and Reil. The second region of the worm is merely the group of plates which connect the hemispheres of the upper cerebellic surface. The third appears to be that collection of transverse plates at the bottom of the purse-shaped notch, which Reil has described more elaborately under the names of single cross commissure, short visible commissure, and long covered commissure. The fourth or inferior region, is the proper worm. It presents, according to the French anatomist, two prominences, one anterior, the uvula (*ugola*) of Malacarne, which he calls the mamillary eminence of the inferior worm; the other posterior, the great body of the worm, which forms lateral productions, to which he applied the name of *lateral expansions*, or *wings* of the great part of the inferior worm.

Reil, however, preferred the distinctions of Malacarne, but in adoption, modified them. He no where mentions an uvula, but describes in its place, 1st, a part to which he applies the name of *spigot* (Der Zapfen) situate between the almonds; and, 2d, the *nodule* (Das Knotchen) which forms the anterior end of the process. He, however, adopts the term of *swallow's nests* (Die Schwalbennester), which Malacarne applied to certain parts of the semilunar valves on each side of the spigot, or between it and the almonds.

This description of the laminated surface of the cerebellum concludes what is requisite to be known on the first or outer surface of the brain, and naturally suggests the consideration of the second or interior, and of the mode in which they communicate.

This second surface is very different from the former. In situation it is interior or central; and its configuration is such, that we would name it the *figurate or symmetrical surface* of the brain. Instead of presenting the uniform eminences and hollows which distinguish the convoluted surface, it is disposed or moulded into definite shapes, which correspond with each other, as they are situate on opposite sides of the middle plane,—or the parts of which, when situate on this plane, are exactly symmetrical. The surface formed by these variously figured objects, bounds what are termed in the common descriptions the *ventricles* or *cavities* of the brain. We shall show that they cannot justly be termed cavities any more than the hollows between the convolutions, but ought to be viewed as continuations of the exterior or convoluted surface. As the several objects of the figurate surface are well known, we shall, at present, avoiding minute description, notice those circumstances only which we think have not been sufficiently studied, or correctly represented, or which we consider necessary to establish our general principle,—that the convoluted surface of the brain and the laminated surface of the cerebellum communicate directly with the inner, central, or figurate surface of these organs.

The commutual or dichotomous region of the convoluted surface is terminated below by a sinuosity, which is formed chiefly by a part of the brain, remarkable in appearance and organization. This, which was named by the ancient anatomists the smooth or polished body (*σωμα τιλλοειδες*, corpus laeve [*]) to distinguish it from those surfaces which were formed by a cutting instrument, appears in the form of white fibrous matter passing transversely between the hemispheres; but is also marked by certain longitudinal lines, first correctly represented by Vicq D'Azyr. The most conspicuous of these, is that which lies exactly in the middle plane, and which is formed by the meeting of the transverse fibres, of which this body, termed by us *middle or central band*, consists. These fibres, which issue like white parallel lines, exceedingly minute, from the substance of the hemispheres, either stop suddenly, or change their direction at this point. It is certain that their sudden termination gives rise to an appearance, to which the expressive but erring epithet of suture (raphe) has been given. On each side of this other lines are remarked, following the same direction. In general, they are situate about two or three lines from the mediar plane; but are in few instances observed to be very regular in their disposition at the anterior end of the middle band. About its middle, however, a very distinct appearance of lines collected into a considerable bundle, may be observed, proceeding backwards to its posterior end. As they advance, they become more distinct, are about 1½ line broad, and of a greyish colour; at the posterior end of the middle band, they diverge somewhat, and, passing over this, proceed in a lateral direction downwards, till they are lost about the spot where the limbs of the brain (crura cerebri, Die Hirnschenkel) issue from the optic eminences. This forms the inner, central, or grey portion of the cylindroid eminence.

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[*] This is the literal translation and the true meaning of the term used by the Alexandrian School; yet the modern anatomists, who would be scandalized to have their love of science or their independence of prejudice questioned, have adopted and continued the unwarranted translation of the ignorant learned at the revival of literature; and Vicq D'Azyr has actually defended their error, and contradicted nature by the assertion, that this is somewhat harder or firmer than the other parts of the brain. "Cette production a *plus de consistence* que le reste du cer-

The posterior extremity of this body, termed the middle or central band (mesolobe of Chaussier), is rounded; and when the membranes have been removed, the surface which forms this rounded end is found to communicate directly with the chamber named third or middle ventricle. This surface is in truth continued forward, and forms the vault or ceiling (fornix),—a point which, though sufficiently obvious, is never noticed in description, or perspicuously demonstrated. The names of callous body and vault are applied in the ordinary works, as if they were denominations of different objects, or rather of different bodies. If they are still to be retained, it ought always to be stated, that they are names applied to opposite surfaces only of the same object. The relations of the round or posterior end of the middle band are worthy of careful examination. The

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veau,— Ce corps un peu plus dur que le reste de la substance blanche du cerveau." Of many scores of brains examined, certainly with a different object than to prove the learning or ignorance of the translators of the Greek physicians, we never could perceive any difference in the consistence of this and other parts of the brain.

handle of a scalpel inserted beneath it will be found to be in the middle ventricle, with the vault above, the conarium or pineal body, and four eminences of the upper surface of the protuberance (corpora quadrigemina) below, and a part of each optic chamber on each side. We have found it very convenient to remove the posterior part of the hemispheres by a transverse section following the plane or level of the hinder end of the central band. A very distict view is thus given of this communication of the convoluted with the figurate surface; and it may be observed, not only in the middle, but on each side. The middle one is bounded above by the posterior end of the middle band and the back part of the vault; below, by the pineal body and the four eminences; before, it is continuous with the middle ventricle; behind, it looks to that space between the convoluted surface of the brain and the laminated surface of the cerebellum, which is occupied by the horizontal part (tentorium cerebelli) of the hard membrane.

On the sides,—that is beyond the margin of the body with the four eminences, a different arrangement is observed. The posterior end of the middle band penetrates into the substance of the hemispheres; but the grey chords which we have already noticed, in pursuing their lateral course, are immediately enveloped in white plates, derived, as we shall see, from the sides of the vault, assuming thus a cylindrical appearance. A body with a free rounded surface is thus formed exactly opposite to the cerebral limbs, which bends in a curvilinear direction laterally and downwards, and is found to be the great hippocampus or cylindroid process. * In observing this curvilinear course, it rests on, and corresponds to, the upper margin of the cerebral limb, as it issues from the optic chamber. It must not, however, be imagined that it adheres to this body; the surfaces both of the beginning or upper end of hippocampus, and of the cerebral limb, are free and unconnected. They are indeed covered with vascular membrane (*pia mater*), which keeps them in apposition; but if this be removed, as it ought always to be, the two surfaces will be found quite distinct, and in no manner connected, unless in corresponding with each other. The whole of this communication may be seen, and is best examined by keeping the organ in its natural position, and viewing it from behind, after the posterior parts of the cerebral hemispheres have been removed. It forms what was described by Bichat under the name of *the great cerebral fissure*. † We do not approve of this denomination, which does not express very accurately the idea intended; and the idea itself, it may may be remarked, does not present a correct view of the natural arrangement. The conformation which we have attempted to describe does not form a fissure, but a regular symmetrical opening, by which the convoluted surface communicates distinctly and directly with that which is figurate.

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* Chaussier.

† Anat. Descriptive, Tom. III.

The next step of the process in the further exposition of this opening or communication, requires the inversion of the brain, and the exposition of the inferior regions of the convoluted surface,—the base of the brain. We have seen the manner in which the convoluted passes into the figurate surface, above and behind the middle band. At the inferior regions, this transition is effected in a mode somewhat different,—and the channel or means by which it is made, is the curvilinear hollow. To understand clearly the relations by which this is established, the demonstrator, if he have not previously done so, should remove in small portions the membrane which covers the optic commissure and tracts, the pituitary peduncle, the pisiform eminences, and which binds together the adjoining convolutions, and especially those which are connected by it to the limbs of the brain. * When the convolutions have been thus exposed, the furrows on each side of the hollow will be found to be very deep, and the eminences proportionally high from the more solid part of the encephalic organ; and the hollow itself will appear almost like the furrow of a convolution on a large scale. Its peculiarity indeed is, that the bottom of the hollow is not convoluted, but presents an extensive uniform space of greyish cerebral matter, on each side of which, but especially behind, the convolutions are very large and distinct. This convoluted part begins insensibly at the outer or lateral edge of the hemisphere, where it is narrow, becomes broader as it advances forwards and inwards; and about one inch from the mesial plane, is particularly distinct, broader, and more spacious than at any other part of its course. It here presents an infinity of holes or orifices of various size in the cerebral substance, not arranged in any regular order, but uniformly found in this situation. Some of these orifices are sufficiently large to admit the point of a small silver probe; but the greater number are more minute, and do not exceed the calibre of a common-sized bristle. The space over which they extend is various, and its figure cannot be accurately defined; it is gene-

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* This process is best performed by the scissors and forceps, with the occasional use only of the scalpel, and with the aid of an assistant with delicate fingers and forceps.

rally equivalent to the half of a square inch. In numerous dissections, we have found it uniformly at this part, and of this size, as nearly as it is possible to estimate in measurement of objects so variable as those of the organs of the animal body. We have not been able to recognise any difference in the colour of this and other parts of the convoluted surface. We suspect, therefore, that Vicq D'Azyr, who, in noticing this part, has called it the white perforated substance (substance blanche perforée), applied the term of colour, without examining the contiguous parts, when denuded of their membranous investments. The same name nearly (lamina perforata) has been given it by Professor Reil. The orifices which characterize this part of the brain admit, from the Sylvian or middle artery, an infinity of bloodvessels, which here penetrate the part denominated the *hook-shaped cerebral band* (Der Haaken-förmige Mark-bündel), described and delineated by Reil. This circumstance renders the adjoining part the most vascular of the whole organ,—a fact which might be shown to be very important in the pathology of the brain.

Anterior to the perforated spot at the extreme verge of the unconvoluted substance, is situate a small pointed eminence, of grey colour, shaped like a triangular pyramid, most conspicuous when cleared of the membrane which covers it. Proceeding backward from this, we remark two slender white lines of different lengths, and following different directions. The inner of these is short; and after a course seldom exceeding four or five lines in the adult brain, suddenly ceases to be visible. The outer one is much longer, follows a curved direction, with the concave part of its course directed anteriorly, and is of a more vivid white colour. These two lines are the generating or initial filaments of the first pair of nerves. Within the perforated spot the curvilinear hollow is generally conceived to be terminated. The unconvoluted space, however, in which it consists, is here directly continuous with the figurate surface of the brain. This space indeed makes here a sharp turn backwards; and having on the inside the long cerebral band termed the optic tract, may be conceived to be bounded by the limb of the brain. This body, with that of the opposite side, will be imperfectly seen at first; and though the end which unites each with the bridge or protuberance is sufficiently conspicuous, the opposite extremity cannot be distinctly perceived. It is indeed covered by a portion of the convoluted surface, the inner and prominent surface of the medio-inferior or spheno-temporal region. This must therefore be gently moved outwards or laterally, and also raised, and, with the aid of the handle of a scalpel, and the fingers of a dexterous assistant, the cerebral limb will be shown to be here crossed or surrounded by the optic tract of the side. The purpose of this part of the exposition shall be noticed afterwards. * But the attention of the demonstrator is to be directed to that part of the convoluted surface which covered the anterior end and outer margin of the cerebral limb, and which has been raised on the scalpel-handle. When gently everted, it is found to present the thin white body named the *tape or fringe* (taenia) of the hippocampus, and if the portion of convoluted brain most anterior or next to the curvilinear hollow be raised and everted in the same manner, the anterior end of this object, termed the *foot* (pes hippocampi), will come into view. If this operation be properly performed, no part will be torn, broken, or displaced; the objects exhibited are free surfaces, not adhering to each other but only in apposition. The white plate or tape of the hippocampus forms, in the natural position of the organ, the outer and lower border of the opening, while the limb of the brain, and after that the outer and lower surface of the optic chamber, forms its inner border.

To obtain an idea yet more clear and distinct of this communication between the convoluted and figurate surfaces of the brain, we have found it convenient to follow the direction of the opening with the scalpel-handle backwards and upwards, until it reach the upper margin of the brain-limb, where it is shown to be merely a continuation of the lateral part of the posterior opening. † This step of the exposition requires cautious and gentle management; and the peculiar curvature which the parts forming the outer border of the communication follow, renders it difficult to avoid laceration or displacement. In general, it is best accomplished on a single hemisphere, from which the cerebellum has been previously removed; but it is desirable to preserve also the whole of the central band, which shows not only the relative connexion of the several parts, but serves also to demonstrate a curious fact in the configuration of the parts forming the outer border of the communication. This is the hippocampus through its entire course, or the body which its figure has led Professor Chaussier to name the cylindroid process.

This body consists essentially of two parts. The first is an inner or central portion, grey in colour, notched or indented in appearance, and, though free at the indented edge, adhering to the cerebral substance by its opposite margin. This is *the grey indented band.*[*] It is as broad or thick as a large crow-quill. This is covered by the outer or second part, which is a broad thin plate of white cerebral matter rolled or folded over the grey indented band, precisely as a map is rolled over a cylinder of wood, and covering it unless removed by art. This last part of the cylindroid process is what is well known, and always described under the name of the tape or fillet of the hippocampus. Its arrangement with regard to the grey indented band is less generally understood, and is indeed never noticed in the ordinary descriptions. A correct idea of it may be formed in the following manner. Divide, by successive transverse sections, the hippocampus from the part named the foot upwards, as far as it can be traced. It will be found that each section presents a central portion of grey matter, enveloped in a thin curled plate of white matter,—which is free at one edge,—the concave one, and hangs over the grey band like a veil, but adheres, by its opposite, to the cerebral substance. It will be perceived, also, that the white plate does not adhere to the grey band, unless at its fixed margin, and that a probe, or scalpel-handle, may be introduced into the angular furrow between them.[†] As the sections approach the upper end of the cylindroid process, a new arrangement is observed. The white plate becomes unusually broad, and is less bent or rolled over the grey band; while the latter, thus less completely covered, instead of following the same direction with the white plate which it does below, is much separated from it, and leaves a considerable angle between the plate and itself. This arrangement is most conspicuous where the process corresponds to the upper part of the optic chamber and the cerebral limb. The white plate of the hippocampus, traced further, is found to coalesce with the margins of the vault, and, tapering as it approaches the anterior end of that body, is finally identified with its substance. The upper termination, or connection of the grey band, is different. When it begins to recede from the white plate, which it does nearly opposite to the optic eminence, it proceeds directly to the rounded posterior end of the central band, over which, and along the upper surface about one inch, it may be traced; and beyond this it is seldom capable of being distinguished from the white matter of which the central band consists.[†] The *grey indented band*, therefore, which forms the inner or central part of the cylindroid process, is continuous, or connected with the upper surface of the central band of the organ; and, if the terms of origin or derivation be admitted, may be said to be derived from it; while the thin white plate which is rolled over the grey band, and gives the hippocampus its cylindrical figure, is connected with the under surface of the same central band, or that part which the usual nomenclature styles the vault (fornix) of the brain.

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* See p. 130, where it is employed to show the geniculate bodies, or posterior eminences of the optic chambers.

† See above, page 130.

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[*] Gordon;—le corps godronnée, Vicq D'Azyr.

[†] This last fact may also be demonstrated without transverse sections, by simple elevation of the white plate by the handle of the instrument.

Such, as nearly as language can represent, is the configuration of the cylindroid process,—an object which it is impossible to know too accurately, if we reflect that it forms the entire outer border of the great communication between the convoluted and figurate surfaces of the brain. It is quite possible that we may not thus succeed in communicating a clear idea of this disposition, however distinct our own conception may be. In such circumstances, we would recommend another contrivance, which we have rarely found to fail in demonstrating and illustrating the facts which we wish to impress. Let a brain, divested of its membranes, be inverted; and, when the opening behind the outer margin of the cerebral limbs has been exposed, let a deep transverse incision between the feet of the hippocampus of each side be made. This incision must not touch the cylindroid process, but, going deep into the substance of the organ, will detach the whole of that part which contains the several objects of the figurate surface from the anterior part of the hemispheres. The portion thus separated will, however, still adhere by the sides to the substance of the hemispheres; and other incisions must be made, following the direction of the outer or lateral margin of the striated bodies (corpora striata) and the optic chambers (thalami optici), until the whole be completely insulated. If these incisions be properly managed, the whole of the middle band, the vault, the cylindroid process, and the white tape, will be left uninjured, and will present a large hollow, surrounded by an extensive border,—the posterior part of which is made by the posterior end of the central band, while lateral divisions are formed by the cylindroid process of either side. The observer will likewise form a very just idea of the situation and connections of the middle band, and will perceive that its inferior surface is the same as that named *vault* or *ceiling*. He will further remark, that the thin white plates (*taeniæ*) which are attached to the margins of this object, and between which is the space improperly named the lyre, extends in a circular and revolute direction outwards and downwards, so as to cover, in the mode described, the grey band of the cylinder, and gives this object its cylindrical aspect.

[†] The importance of this object in forming the outer border of the great communication, will render it not improper to glance at the history of its discovery. Vicq D'Azyr is supposed to have been the first anatomist who observed and described its site and appearance with accuracy; and we find him, in a Memoir of the Royal Academy of Sciences in 1787, detailing its peculiarities with some minuteness, under the name of "Bord externe et dentelé de la Corne d'Ammon." He subsequently represented it, in his great work in the year 1786, under the name of grey, cortical, external, notched portion of the Horn of Ammon; and speaks of it familiarly enough by the term of corps godronnée. Bichat, however, whose descriptive anatomy was published considerably later, though his accurate and precise manner prevented him from overlooking it, and led to give a sufficiently minute account of it, boldly asserted, that it had been entirely neglected by authors. Dr Gordon, of this place, who was quite aware of this oversight in Bichat, took special care, in demonstrating it, to notice the merit of Vicq D'Azyr; and he has mentioned this fact shortly in a note, in his description of the grey band. None of these eminent anatomists have been altogether right in this matter; and the truth is, that neither was Vicq D'Azyr the first who delineated this body, nor was Dr Gordon's assertion that he did so, quite well founded. The mistake might have been prevented, if either of them had looked into a work which contains much information on the progressive literature of cerebral anatomy, and the neglected obscurity of which we never could explain. The grey indented band is distinctly represented by Pierre Tarin, in the second engraving of his Adversaria; and although this had been published at least thirty years before the engravings of Vicq D'Azyr, yet it was requisite for the latter writer to make it the subject of a particular Memoir, and Bichat to write a description of it, before it seems to have been known as a part of the organ.

The correct understanding of the outer border of this communication, should be followed by an equally just idea of those parts which form what we must name its inner border. These are more numerous and rather more complicated ; but frequent examination with a little attention, will enable the student to overcome all the difficulties with which they may be attended. The parts contained in the mass of brain removed, as we described in the last section, will contribute also very much to render the knowledge of this border easy and intelligible. We have already said, * that at the posterior end of the middle band, the inner or lower border is formed by the four eminences (c. quadrigemina) situate in the upper surface of the thick mass named *protuberance*. On each side of these, the inner border is formed by successive portions of the optic chamber, until it has reached the inferior region of this body, where the outer and lower margin of the limb of the brain becomes the border of the opening. To render this arrangement more intelligible, we shall here state some circumstances of the figurate surface, which are either omitted in the ordinary descriptions, or are mentioned in an indistinct and desultory manner. Its principal objects are familiarly known ; and there are certainly none who can be ignorant of the situation of the striated bodies, the optic chamber, the semicircular chord between them—and other similar objects. The posterior eminences, or the optic chambers hold the most important place, and their connexion and relative situation render them much more interesting than they are usually made in works of descriptive anatomy. Connected before and on the outside to the striated body by means of the double semicircular chord, (centrum semicirculare geminum), each optic eminence presents four free surfaces,—the upper, the inner, the posterior, and the lower. The upper, the first, is gently rounded and of white colour; its figure and limits not easily defined; the outer margin is bounded by the circular band, which even passes on anterior to it, so as to form its boundary in that direction also; behind, it is less distinctly limited, unless by the appearance of a considerable prominence, which has been generally named the posterior tubercle of the optic couch. The inner margin of the upper surface is most distinctly marked by a small sharp grey line, which beginning insensibly at the anterior part of the body, becomes more distinct as it extends backward, and is ultimately found to bend gently toward the mediar plane. There it unites with a similar elevated line of the opposite optic eminence; and to the point of union is attached a small conical body with a minute point, of a grey colour, and of a shape like that of the pine-apple. This is the object improperly termed pineal gland; and the minute linear eminences or tracts, which form the inner edge of the upper optic surface, have been named peduncles of the pineal gland. The inner surface of the optic couch or chamber presents nothing important, save the small portion of soft cerebral matter, which unites it to the similar surface of the opposite body. Its posterior edge, however, is terminated by the cerebral limb of that side, and the lower edge meets that of the opposite one, and is connected to it by a portion of brain, which, examined in this manner, has received no name, but forms the lower part of the middle ventricle. The portion of brain which corresponds to it on the outside (base of brain), has been named the bridge of Tarin. It is the triangular space between the limbs of the brain, which has been called by Vicq D'Azyr *the pit of the oculo-muscular nerves.* It may be named the triangular intercrural hollow.

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* See page 130.

The posterior surface of the optic eminence is the most important, but the least understood. It is so intimately connected with the inferior, that we cannot easily distinguish them in description. Santorini, we believe, was the first who observed that the posterior part of the optic couch was terminated by a projection or process, which he named the jointed or geniculate body, and which, he had likewise the merit of observing, gave rise to the optic nerve.[*] More recently, Malacarne remarked, that this region was rendered unequal by a tubercle, from which the nerves seemed partly to rise.[†] About the same time it was noticed by Vicq D'Azyr, that on the side, a little behind and below, there were three superficial tubercles, of a rounded figure and disposed in a triangle, and that one of them must have been the body mentioned by Gunz and Bonhomme, and of which Haller avowed his ignorance. He goes on to mention a slight prominence near the upper of the four eminences,—of a white colour, but grey exteriorly, communicating with the optic tract;[‡] while the external-lateral region of the optic couch presents several similar eminences, but less considerable. The notice given by Bichat is still more brief and less satisfactory. "Tout a fait en arriere cette partie inferieure offre une on deux saillies, et se continue avec les tubercules quadrijumeaux." The great fault with which we feel ourselves obliged to charge each of these authors, is the slovenly and inaccurate manner in which they seem to have examined this part of the organ, the vague and unprecise terms in which they have spoken of it, as if its existence were apocryphal; and the neglect of either availing themselves of the researches of former anatomists, or supplying their defects by personal investigation. More recent accounts have been given by Soemmering and Gordon, both of whom describe two eminences in this situation, and termed the outer and inner geniculate bodies. Yet we know not whether their descriptions are read, or whether they are quite intelligible. That of the former author is brief and precise, but it is not quite so satisfactory as we could wish. That of the latter is more minute; but in some respects it is either not clear, or it does not correspond with what we have ourselves seen. On this point, however, we would be understood to express ourselves with the deference which we shall ever feel to an ingenious and lamented instructor.

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[*] Quod tamen per repetitas diligenterque institutas disquisitiones ab eadem potius membrana eos non proficisci, saepius vidisse visus sum; verum tum ab eorum Thalamorum interiori parte, tum a *quodum velut geniculato corpore circa corundem Thalamorum posteriora locato adjunctoque*, cujus corticalis seu cinerea interior substantia est, medullaris et candida exterior facies luculentius prodire, sum assecutus: Ad horum tamen exortus locum a latere prominentiarum quod natiformes nuncupantur, conspicuus medullaris tractus transversim sic Thalamis conjungitur, ut vel in eorum substantia disjiciatur, vel inflexus caeterisque fibrillis involutus ad Nervorum opticorum exortum accedat, conjicere quidem, decernere autem minime valui. Observat. Anatomic. Joan. Dom. Santorini, cap. iii. § xiv.

[†] La stessa faccia superiore—è coperta d'una lamina midollare tennissima ed ha disuguale la posteriore estremità per un bernacolo irregolare, da cui sembra che in parte nascano i nervi ottici.—Articolo, vii. 68.

[‡] Recherches sur la structure du Cerveau, &c. xi. Memoires de l'Academie Royale des Sciences, Année 1781.

The confusion and uncertainty which prevails on this piece of cerebral anatomy, have induced us to adopt a method which we have experienced to be more certain in unfolding the objects in question, and which we think may be safely recommended to the practical anatomist. Let the brain be examined from below, and let the examination commence with the optic tracts. That the acceptation of this term may be quite definite, we begin with observing, that the optic nerves should be in accuracy counted from the commissure only. The cerebral extremity, or origin, as it is named, of a nerve, should be reckoned from that point only at which it is quite free in its whole circumference from the organ. This happens to the optic nerves at the anterior part of the commissure only; for behind this body, the longitudinal bands called *tracts* adhere by one side (the outer), to the cerebral substance. A narrow angular sinuosity, admitting the point of a small probe, is found at the inner or mesial edge of the tracts; but beyond this, they adhere indissolubly. As they pursue their course backward and outward, they reach the limbs of the brain, the inferior surface of which they cross, still adhering by their outer edge, but more extensively. Here, likewise, they are sensibly broader than at their anterior end. When they have passed completely the limbs of the brain, at which they are about the breadth of three lines, they begin to present a linear furrow or depression, which extends in their long direction about one half or three-fourths of an inch, and thus divides the tract into an *outer* and an *inner* limb. This depression, however, does not divide the tract equally, but leaves the outer broader than the inner limb. It is insensibly terminated at the upper surface of the optic eminence or chamber; and its last part (about three or four lines) contemplated with care, will be found to separate two spheroidal eminences which are respective terminations of the outer and inner limbs of the tract. The outer of these eminences is the largest and most prominent, and is the body mentioned by authors as the posterior tubercle of the optic chamber. It is the body which Santorini named the geniculate (corpus quiddam geniculatum),—an epithet for which it is not easy to account, unless we suppose that his fancy had likened it to the appearance of a bent joint, especially that of the knee, to which, if viewed laterally in connexion with the tract, it bears some remote resemblance. It is broader, larger, and more convex than the inner eminence, and is evidently the chief origin of the optic tract. The eminence on the inside of the linear furrow is much smaller, and often less distinctly marked than the outer. In ordinary circumstances, it may be described as an eminence of cerebral matter contained between two circular segments, in such manner that its longitudinal extent, which is nearly vertical, is greater than its transverse. It is obviously connected with the smaller limb of the optic tract. Whether this body actually takes its origin from the four eminences (corpora quadrigemina), depends on evidence of a different description, and may probably fall under our consideration before the conclusion of these inquiries.

The inner or mesial side of the geniculate bodies, is contiguous to the four eminences, and is separated from them by a linear furrow. These eminences occupy the upper surface of the protuberance, and partly that of the limbs of the brain; and the linear furrow marks the point at which the limbs issue from the substance of the optic couches. The lower surface, indeed, of these bodies, presents the long thick cylinder of the limbs; and while they occupy this conspicuous situation, and considerable proportion of the lower surface, it is impossible to assign them more cerebral space above, than that occupied by the upper two of the four eminences (nates), and the adjoining part of the optic couch.

This view of the objects occurring in this region of the figurate surface, will show the formation and arrangement of the inner border of the communication. It will be seen that it consists of successive portions of that figurate surface, proceeding from the mesial plane, on each side to the lower and outer margin of the cerebral limbs. It will be seen, that if we begin at the longitudinal furrow of the four eminences, we find first the upper eminence of one side; then the contiguous part of the optic couch; then the greater geniculate eminence; after this, as the cylindroid process pursues its winding and revolute course, the outer part of the cerebral limb; and, lastly, the outer and lower angle of that limb.

Not only, however, does the convoluted surface of the brain communicate with the figurate one, but the laminated surface of the cerebellum communicates with the analogous surface of that organ, and thus with the great figurate surface. This communication takes place on the middle plane, below the transverse or middle plates which form the pyramid, spigot and nodule, and between these bodies and the restiform processes. This is the fourth ventricle of anatomical writers; and as it has never been denied that it communicates with the outer surface of the cerebellum, we only notice it here as a part of the general views of cerebral anatomy which we humbly conceive are more true, reasonable, and correct, than any which we have yet seen. We trust that we have succeeded in showing, that the whole organ may be viewed as a body of peculiar substance, formed into two surfaces, or placed, as it were, between two surfaces; that these surfaces, though distinct, are not separated by any barrier, but pass gradually and directly into each other; that these surfaces differ only in situation and in mode of configuration,—the one being exterior, and moulded into convolutions, or into concentric and parallel plates; the other interior, and fashioned into symmetrical and corresponding figures. These views, in short, would suggest a division of the central nervous mass, as it has been named, very different from any hitherto adopted, and much more accordant with its organic characters. According to the principles thus unfolded, the parts contained in the skull and vertebral canal may be said to consist of three great divisions;—the convoluted, or what is named the brain proper; the laminated, or what is termed the small brain (cerebellum); and the smooth or uniform portion contained in the vertebral canal.

This long, and perhaps tedious, examination of the configuration of the brain, ought, according to our plan, to be succeeded by an examination of the state of our knowledge of its intimate structure. The necessity, however, of attending to other subjects, compels us here to make an abrupt pause, and defer farther inquiry till a more convenient opportunity shall occur for resuming a subject which requires both space and deliberation.

II.

1. *Recherches Physiques, &c.* Researches on the Properties and Functions of the Nervous System in Vertebrated Animals. By M. Flourens. *Archives Générales de Médecine, Juillet* 1823.
2. *Rapport fait à l'Académie, &c.* Report delivered to the Academy of Sciences at Paris, on the Experiments of M. Flourens. By Baron Cuvier, and Others of its Members. *Journal de Physiologie, Octobre* 1823.
3. *Expériences, &c.* Experiments on the Functions of the Nervous System. By Professor Rolando of Turin. *Ibid. Avril* 1823.

4. *Analyse des Expériences, &c.* Analysis of Experiments on the Nervous System of Man and Animals, published in Italy in 1805, and repeated in France in 1822. By M. Coster. *Archives Générales de Médecine, Mars* 1823.
5. *Recherches Expérimentales, &c.* Experimental Researches on the Nervous System. By M. Fodera. *Journal de Physiologie, Juillet* 1823.
6. *Expériences sur les Fonctions, &c.* Experiments on the Functions of the Roots of the Spinal Nerves, and on the Seat of Motion and Sensation in the Spinal Cord. By M. Magendie. *Ibid. Tom. II. & III., passim.* 1822-3.

No subject has excited deeper and more universal interest among the present physiologists of France, than the Properties and Functions of the Nervous System. Many individuals, both of rising talent and of established fame, have, within these few years, devoted their labours to experimental inquiries on the subject. And here, as well as in other branches of physiology, no sooner have hypothetical fictions been laid aside, and experimental reasoning made the only legitimate mode of investigation, than the science has been enriched with many interesting facts, and several brilliant discoveries. The honour of these discoveries belongs chiefly to the physiologists whose names are placed at the beginning of this article. But it is exceedingly difficult to assign to each of them his precise share of merit. For as their researches have been carried on about the same time, guided by the same principles, and directed towards almost the same objects, it is not wonderful that they have often encroached on one another's territories, and that some of them have occasionally arrived at results almost precisely similar.

This concomitance of pursuits among so many persons is not, we apprehend, the effect of mere chance. At one time, it seems to have been an understanding among the scientific in general, that, when a man was lucky enough to strike out a new path of inquiry, had favoured the world with some of his discoveries, and pledged himself to prosecute them, he was to be allowed to pursue his course without disturbance or encroachment. And such, we believe, is still the understanding in Britain. Not so in France: There, as soon as any one has opened a new field of discovery, and gathered up the produce of a part of it, his brethren on all hands conceive themselves entitled to enter it also, and to partake in the harvest along with him. We shall not attempt to legislate as to the right of property under these circumstances. Although the practice is probably unfair, as regards the original discoverer, and is certainly at variance with several well-known analogies; yet no one can entertain a doubt that it must benefit science, in the first place, by exhausting the field of inquiry much sooner and much more completely, and next, by furnishing a good criterion of the value of all their researches, in the contrast and comparison of them with each other. Some of these conceptions are conspicuously illustrated by the history of the inquiries, of which we now propose to lay an abstract before our readers.

About the beginning of 1822, M. Flourens, a young French physician, laid before the Parisian Academy the first fruits of his physiological labours, being an Inquiry into the Properties and Functions of the Nervous System. In this work he undertook to prove, that the nerves, spinal cord, medulla oblongata, and corpora quadrigemina, are alone the seat of those impressions which give rise to muscular contractions; that the spinal column combines or generalizes these contractions, so as to produce motion of the joints; that the cerebellum regulates the motions of the joints, so as to produce the actions of running, walking, standing, flying, and so on; and that the cerebral lobes are the seat of sensation and volition. His doctrines soon became the object of much interest in the medical coteries of Paris, partly on account of their intrinsic importance, and partly because it was discovered that some of them had been advanced thirteen years before, by an Italian of the name of Rolando, then professor of medicine at Sassari in Sardinia. The obscurity of Rolando's situation had kept his researches entirely out of the view of the French school, till the publication of Flourens' discoveries. They were then displayed with much ostentation by a Dr Coster, one of Rolando's former pupils; and of course the young Frenchman was at once accused of plagiarism. The charge of dishonesty, however, appears to be wholly without foundation; and of this the memoirs of the two rivals afford internal evidence. For, although many of their experiments closely resemble each other, the conclusions which Flourens has drawn from them are widely different from those of Rolando. In fact, Flourens might have quoted all Rolando's treatise, without materially diminishing the value of his own discoveries; and in such circumstances, it does not appear likely that he would venture on the commission of the crime of which he has been accused.

The memoir laid by him before the Academy of Sciences, contains but a part of his observations; and he has announced his intention of extending them, and of prosecuting his researches throughout the rest of the nervous system. He seems, however, to have been prevented from executing this purpose by a long and severe illness; and, meanwhile, his countrymen, in conformity with the principle already mentioned, have repaired in flocks to the field he has thus temporarily relinquished. The object of the subsequent observations is to present a combined view of all their inquiries and discoveries, and so to attempt an estimate of our present state of knowledge with regard to the functions of the nervous system. Our observations, as justice demands, will be arranged according to the order pursued by M. Flourens.

M. Flourens very properly sets out with an attempt to reform the present nomenclature of the nervous properties and functions. But, unfortunately, he concludes by leaving it involved in greater confusion than ever. When a nerve is pricked in a living animal, two distinct phenomenons are observed to follow,—contraction of the muscles it supplies, and the sensation of pain. But the nerve itself does not contract; neither does it experience sensation. For if a ligature be applied on each side of the part that is to be pricked, no contractions take place in the nerve or elsewhere; and the animal does not express pain. The nerve has therefore the property of receiving a peculiar impression, which is conducted in both directions along its course, producing contractions at its extremities, and sensation somewhere at its origin in the great nervous centre, the brain and spine. Glisson was the first who gave to the phenomenon of muscular contraction, under the application of stimuli, the name of *Irritability;* but he confounded with it under the same name, the phenomenon of contraction from simple elasticity. Haller afterwards restricted it to the property of contracting briskly, inherent in the muscular fibre only. Bichat more lately substituted for it the term *Contractility;* which, however, he again extended so as to comprehend the property of elastic contraction. The word *Sensibility* was originally applied to the property, peculiar to the external nerves, by which we become acquainted with the presence and qualities of surrounding objects. The physiological system of Stahl, (who thought that the soul governed every action in the body, voluntary and involuntary), and likewise that of Bichat, made it include "every nervous cooperation accompanied with motion, even though not attended with perception." Farther, owing to the ambiguity of the words in ordinary language, the nerves have been too generally said to be irritable and sensible, though they are merely organs for conveying the impressions which are to call forth these properties elsewhere. M. Flourens, after noticing imperfectly these vices in our nomenclature, proceeds to fix the meaning of the terms he is to employ in his investigations. The term *contractility* he uses to denote the property, inherent in muscular fibre only, of undergoing brisk contractions under the application of stimuli; and the term *sensibility*, to imply the property of experiencing *sensations*, according to the sense in which that word is familiarly used in metaphysics. This is all very correct. But he has made use of the much tormented word *irritability*, in the most arbitrary and unjustifiable manner, to indicate the property possessed by the nerves, of receiving the peculiar impressions, which give rise to sensation and contraction; " the property of provoking sensation and motion, without experiencing them, is precisely what I call irritability." The idea of giving a separate name to this property is certainly a happy improvement in physiology. But his choice of a term could hardly have been more unfortunate. The Commission appointed by the Academy of Sciences to report upon his Memoir, after noticing the impropriety of selecting a term already so ambiguous, have suggested, that, instead of calling the nerves irritable, he should say they are the conductors of sensation and irritation. But, besides necessitating a constant and irksome circumlocution, this phrase is equally inexact with the other; for, assuredly, before a nerve can conduct sensation or irritation, it must receive it; which is quite equivalent to pronouncing it sensible and irritable. In the remarks which follow we shall venture to substitute the term *impressibility* for that of irritability chosen by Flourens; by no means presuming that it is the best which can be found, but trusting that it cannot lead to any mistake, either by implying a theory, or by indicating any other nervous property.

These indispensable preliminaries having been settled, we shall proceed to analyze the Memoir as it is contained in the Archives Générales for last July.

M. Flourens first shows, by the familiar experiment of tying two ligatures on a nerve, and pricking it above, below, and between them, that the nerves are neither contractile nor sensible, but that they are essentially impressible (irritable). He next shows, by dividing the spinal cord in two places, and pricking it above, below, and between them, that this part also of the nervous system is impressible, but can neither contract nor feel. From the other results of this experiment, results which are so familiar that we need not enumerate them, he also concludes, that " the spinal cord is the organ by which is effected the dispersion or generalization of impressions (irritations)." This language is mystical. The Commission of the Academy seem to think that he holds the spine to be the medium, through which sympathies are established; and very properly deny that his experiments can fairly lead to such an inference. In another part of the Memoir, however, he states that the spine is the organ by which the muscular contractions are combined so as to produce motion of joints or limbs; and he proves this by referring to the general motion excited in the hind-legs by irritating them, after the spinal cord has been divided in the middle of the back.

The next object of his experiments is to discover in what parts of the nervous system the property of Impressibility resides. And he proves that it belongs only to the nerves, spinal cord, medulla oblongata, and corpora quadrigemina; and that the lobes of the brain, even the corpora striata and optic thalami, and likewise the whole mass of the cerebellum, do not possess it. In the first place, the nerves everywhere possess it. The most extreme subdivision which can be traced, if pricked or otherwise injured, gives rise to contraction of the muscles it supplies, and likewise to the sensation of pain. Secondly, this property does not extend higher up than the corpora quadrigemina. The whole brain proper and cerebellum may be pricked, or slashed, without exciting muscular contractions anywhere, and without causing pain; and they may be removed altogether, without occasioning the loss of motion, or even permanently diminishing it. But if the superior corpora quadrigemina be pricked, some agitation is caused; and the nearer the instrument approaches to the medulla oblongata, the more distinct is the expression of pain, and the more manifest are the muscular contractions. Rolando too had observed, that injuries of the brain proper produced only a kind of stupor, but that convulsions supervened when the optic thalami and corpora quadrigemina were intersected (*Arch. Gén.* I. 364.) He confesses, however, that he could not ascertain the precise effects of injuries of these organs (p. 366); and he nowhere draws any inference as to the latter being the upper limit of nervous Impressibility. The facts advanced by Flourens, besides receiving the solemn sanction of the Academy's Commission, have been since abundantly confirmed by Fodéra, who observes (*Journ. de Physiol.* III. 192), that in the mammalia the cerebral lobes may be cut, pricked or torn, without exciting pain or muscular contractions; but if the instrument be carried farther back than the optic thalami, so as to intersect the superior corpora quadrigemina, both of these phenomena at once take place. And he likewise found, that wounds of the cerebellum were as little capable of causing pain or contractions, though they were followed by certain singular motions, the import of which will be noticed presently. As to the effect of pressure on the Encephalon, the same author, M. Fodéra, has furnished the clearest evidence, that pressure on the cerebral lobes or the cerebellum causes agitation, stupor, or apoplexy, not by affecting these parts themselves, but by compressing indirectly the medulla oblongata. For the brain or cerebellum may be squeezed laterally till it is even crushed, without any lethargy supervening. Some degree of stupor indeed is observed, when the anterior lobules of the brain are pressed downwards, and still more when the posterior lobules or the cerebellar lobes are similarly treated; but the same effect is produced if these parts be previously removed, and a firm paste substituted for them; and therefore no doubt can exist that the stupor is owing to indirect compression of the medulla oblongata (Ibid. 195.) These facts will serve to explain several obscurities which hang over some subjects of investigation to be noticed hereafter.

M. Flourens then turns his inquiries to the seat of Sensation and Volition, that is, the part of the nervous system at which impressions proceeding from its extremities must arrive, before they can produce Perception and subsequent efforts of the Will. The faculties of Perception and Volition do not reside in any part of the nervous system, which we have found to be endowed with Impressibility. For, if a section be made at any point betwixt the superior corpora quadrigemina and the extremities of the nerves, the animal feels no impression made below the section. These faculties must therefore reside somewhere higher up in the encephalon. And M. Flourens thinks he has established by experiment that they are situated in the cerebral lobes. The position seems to be satisfactorily established, so far as concerns the senses of sight and hearing; but his experiments do not by any means warrant him in extending it, as he has done, to the other senses, and to volition.

When the cerebral lobes are removed in any animal, it evidently loses at once the senses of sight and hearing. If, on the other hand, the cerebellum be removed, the brain proper remaining entire, these senses are unimpaired. If only one hemisphere of the brain proper be cut out, the sight of the opposite side only is lost. It is an interesting fact, however, and worthy of special notice, as illustrating what has been said concerning the seat of Impressibility, that, although the sight is lost, the contractility of the iris continues unaffected. When the cerebral lobes are pricked even so deep as the optic thalami, the iris remains unmoved; if they are cut out, it is not paralyzed. But the moment the superior corpora quadrigemina are touched, it contracts convulsively; and whenever they are dug out, care being taken to leave no portion of them adhering to the optic nerves, it immediately dilates, and entirely loses its contractility. These facts, conformable with the assigned limits of nervous Impressibility, show the incorrectness of the generally received opinion, that the integrity of the optic thalami is essential to the integrity of the motions of the iris. This opinion had been derived from the well-known effects of pressure on the optic thalami and their vicinity. But it is easy to see that pressure can hardly be produced there, without also indirectly compressing the optic nerves. The observation of Flourens, that the sensation of visible objects and of sound resides in the lobes of the brain proper, is entirely new. It is included, indeed, in a more general proposition to be stated immediately, to which Rolando had been led by his experiments. But the Italian nowhere lays it down explicitly; and besides we shall find, that his experiments are liable to a serious fallacy, in consequence of which he cannot be held to have established even the general proposition alluded to. Moreover, he does not even hint that the optic thalami are not necessary to the contractility of the iris. On the contrary, his section was always made before the thalami; and he expressly mentions that, when these bodies were cut into, the pupils dilated, and ceased to be contractile (*Archives*, I. 368.) As to the correctness of M. Flourens' facts and inferences, we can only say, that his experiments were repeated before a Commission of the Academy of Sciences, consisting of Cuvier, Portal, Berthollet, Pinel, and Duméril, who agree that he had made his proposition good.

Besides the effects just mentioned, Flourens observed, that the loss of the cerebral lobes was followed by a kind of lethargy or deep sleep; from the peculiar symptoms of which he draws the conclusion, that this part of the nervous system is the seat, not only of all other sensations, but likewise of Volition. As that conclusion, however, does not seem to be so legitimate, it is necessary to state, more in detail, the principal experiments from which it has been drawn. When the cerebral lobes were removed in a pigeon, without injuring the corpora quadrigemina or cerebellum, the animal was immediately seized with more or less feebleness; which effect, however, gradually diminished, till it soon became evident that motion was nowhere materially weakened. But the senses of seeing and hearing, as formerly noticed, were extinguished, and a state of stupor was induced, resembling profound sleep. The animal remained calm, and as if abstracted; did not move of its own accord; and, when it encountered an obstacle, struck it again and again, without ever trying to avoid it. Yet it preserved its equilibrium, struggled if held, resisted every effort made to open its beak, swallowed water dropped into its beak, walked about when pushed, and flew when thrown into the air; nay, the slightest irritation evidently teased it (*Archives*, II. 352). Analogous results were obtained with frogs. Now, these phenomena by no means justify the inference, that all sensations, as well as volition, are lost. They are impaired; but the symptoms unequivocally show, that the animal could both feel and will. Yet sensation and volition certainly are impaired; or perhaps we should rather say, that the memory seems to be materially injured, so that impressions made on the organs of sense excite but obscure or transient perceptions, and consequently but feeble efforts of volition.

Such is nearly the amendment which the Commission of the Academy advise M. Flourens to make on the nature and terms of his inferences. They remark, that he has amply established his doctrine with regard to the seat of the sensation of sight and sound; but that it was impossible he could put it to the test as regarded the senses of taste and smelling; and that it is not at all extensible to the sense of touch throughout its numerous modifications. " We should be content with holding," say they, " that the cerebral lobes are the sole receptacle, where impressions on the senses of sight and hearing become perceptible to the animal. If we were to add any thing to this concession, we should say, that they are also the part where all sensations whatever take a distinct form, and leave durable traces and recollections; in a word, that they are the seat of memory, by which property they supply the animal with the materials of judgment." (*Journ. de Phys.* II. 381.)

The experiments of Rolando, conducted nearly in the same manner with those of Flourens, had also led him to form the same opinions as to the functions of the cerebral lobes; namely, that they are the seat of sensation and volition. He found, that, after they were destroyed in cocks or crows, these birds generally stood motionless for hours together, as if plunged in deep sleep, and might be pricked without giving any sign of sensibility; but that they preserved their equilibrium, sought for support, and could be made to walk when struck violently. Similar effects were also occasioned in kids, lambs, and guinea-pigs. Although in general, therefore, he obtained even more imperfect signs of sensation and volition than M. Flourens, nevertheless his own account of his experiments supplies sufficient evidence that neither of these faculties was extinguished. The differences observable betwixt the results obtained by the two physiologists are probably to be attributed to varieties in the mode, or exact seat, in which the section of the brain was performed. Magendie believes that Rolando, in his experiments, did not always prevent or make allowance for the effects of pressure from effused blood; and the same observation may perhaps be also applied in some degree to those of Flourens. For he observes that his animals, after the abstraction of the cerebral lobes, did not move of their own accord; whereas Magendie found that, on taking care to prevent extravasation and compression, young rabbits, as well as young jackdaws and magpies, ran about and jumped vigorously and spontaneously, after the removal of every part of the brain a little in front of the optic thalami (*Journ. de Physiol.* III. 155.)

Our authors do not always observe due precision in describing the exact seat of the section of the cerebral lobes; and it is likely that even trivial deviations will cause essential differences in the phenomenon. Magendie remarked, that if the section was made immediately before the optic thalami, and not at a short distance before them (as in his experiments just noticed), the vigorous motions of the animals were at once suspended, they fell down on the side, the head was drawn back, and the limbs became rigidly extended (*Ibid.* 156.) Rolando, who generally made his section at the same point, seems also to have occasionally observed catalepsy ensue (*Archives*, I. 365.) Magendie farther adds, that when the seat of section was then carried backwards, so as to remove the optic thalami, the limbs regained their suppleness, and the head its straight direction; motion, however, continuing suspended. From these facts he conjectures that the optic thalami are somehow connected with the power of motion; and he proposes to commence an inquiry, with the view of determining the nature of this connection. Now, it is obvious that, if his experiments are correct, and his conjecture should turn to account, M. Flourens must modify his doctrine as to the limits of nervous Impressibility. Yet this latter physiologist nowhere noticed the differences observed by Magendie. He does not always mention, indeed, the exact seat of operation. But judging from his doctrine, and the description he has sometimes given of the place of the section, he must have removed the brain in all cases behind the optic thalami. And farther, it will be remembered, that his conclusion as to the limits of nervous Impressibility, has been verified by several other experimenters, and sanctioned by a Commission of the chief physiologists in the Parisian Academy.

We come now to mention the most interesting and extraordinary of all M. Flourens' discoveries, namely, that the cerebellum is the Regulator of Motion; or, in other words, the organ by which the muscular contractions, previously combined by means of the spine to constitute movements of the joints, are further associated and regulated, so as to produce the actions of standing, walking, running, leaping, flying, swimming. His experiments on this head are very pointed. When a slice is cut away from the cerebellum of an animal, the injury is at once followed by great weakness. This differs in degree with a variety of circumstances, being greatest when the quantity removed is greatest, and always more striking in the mammalia than in birds or reptiles. The same effect we have already seen to follow the removal of the lobes of the brain. It evidently depends on the sympathetic connexion of the several parts of the nervous system, in consequence of which no part can be injured without a shock being imparted to the rest. But in a short while the animal regains its strength; and then the senses are found to be entire: It shrinks from the slightest threat or violence offered to it, and the motions of the limbs are generally vigorous and frequent; but its movements are exceedingly irregular, and, as it were, embarrass each other; and though volition is evidently exerted, no corresponding act succeeds; it strives to escape when irritated, yet it can neither fly, nor run, nor leap, nor walk; nay, when the whole cerebellum is lost, it cannot even stand. If that organ be removed gradually in successive thin slices, the progressive circumscription of the locomotive actions is very remarkable. On removing only the first layers of the cerebellum in the pigeon, the sole effect produced is some weakness and a kind of hesitation in its gait. When the sections have reached the middle layers, it staggers much, and assists itself in walking with its wings; yet it sees and hears perfectly, seems cheerful, and does not express pain. The sections being continued farther, it is no longer able even to preserve its equilibrium without the assistance of its wings and tail; its attemps to fly or walk resemble the fruitless efforts of a nestling, and the slightest touch tumbles it over. At last, when the whole cerebellum is removed, it cannot support itself even with the aid of its wings and tail; it makes violent efforts to rise, but only rolls up and down; then fatigued with struggling, it remains for a few seconds at rest on its back or belly, and then again commences its vain struggles to rise or walk. Yet all the while its sight and hearing are perfect; the slightest noise, threat, or other stimulus at once renews its contortions. In these struggles and contortions there is not the slightest appearance of convulsions. The same effects were observed by Flourens in the guinea-pig. When the last layers of the cerebellum were removed, it lost the power of walking or standing, lay down on its belly, moved its legs as if running, and made vain efforts to rise. The inference he has drawn, therefore, from the foregoing facts is, that the cerebellum is the organ by which all the locomotive actions are regulated.

This discovery is entirely original. Rolando, in particular, for whom the merit of it has been claimed by his pupil Coster, had not made the most distant approach to it. On the contrary, he maintains that the cerebellum is the organ on which motion depends altogether; and at first sight, his experiments are conclusive enough to this effect. For he found that, when he removed the half of the right lobe of the cerebellum in a cock, it was immediately struck with palsy of the same side, so complete that it could not execute a single movement with the right wing or leg; and when both cerebellar lobes were removed in a kid, it lost the power of motion every where. Three circumstances, however, render these experiments inconclusive. First, he does not seem to have made allowance for the temporary debility which immediately follows the injury. Secondly, his own expressions make it evident, that he could not prevent the effusion and accumulation of blood in the cavity of the skull; so that the effects of the loss of the cerebellum were complicated with those of pressure on the medulla oblongata. And, lastly, he may with justice be suspected of inaccurate observation; for he remarks, that the kid lived twenty-four hours without the cerebellum, in a state of complete paralysis of both sides; a state, in which it is not easy to conceive, how the respiration could be carried on so long. The doctrine laid down by Flourens has moreover been sanctioned by the Commission of Inquiry appointed by the Academy of Sciences. It has also been more or less confirmed both by Magendie and by Fodéra. The former, after noticing the extreme difficulty of making a conclusive experiment on the cerebellum, remarks, that all he had been able to make out was, that every severe injury of it took away the power of advancing, and excited a constant tendency to walk, run, or swim backwards (*Journ. de Physiol.* III. 157.) Fodéra also found, that animals thus mutilated sometimes showed the same tendency to retreat; but that more commonly they fell down, agitating their limbs and unable to rise, and occasionally staggered, jumped, and tumbled in a strange irregular manner (*Ibid.* 195). These results are so similar to the phenomena witnessed by Flourens, that his facts may be admitted as genuine; and in that case we cannot help allowing, that his inference as to this function of the cerebellum is legitimate.

Thus, then, the property of nervous Impressibility is limited to the corpora quadrigemina, medulla oblongata, spinal cord and nerves; the integrity of the optic thalami is not essential to the Contractility of the iris; the sensations of sight and sound reside in the cerebral lobes, and there also all other sensations acquire distinctness and durability; the spinal cord combines the muscular contractions so as to produce motion in the joints; and the cerebellum regulates these movements, and unites them so as to constitute the actions of standing and locomotion;—such are the discoveries of Flourens. On a subject of so much nicety and intricacy, the medical world will naturally receive, not without much wariness and hesitation, doctrines so precise, so important, and so unexpected. We have endeavoured in the foregoing remarks to appreciate their value impartially, so far as their novelty will permit. We shall not now attempt to offer any additional arguments for or against their validity; being satisfied that, as they have been deduced by a system of experimental reasoning, nothing but new experiment can be brought forward on either side with effect. Meanwhile, however, it may be right to notice shortly the arguments which have been levelled against them by a man, who, since his own interest is deeply staked in their destruction, may be justly regarded as their most mortal antagonist. M. Gall, incensed to find his organs of love, philoprogenitiveness, and many other propensities and noble faculties, all snatched from his hands to make up one poor, paltry machine for regulating the baser bodily motions, and another equally contemptible for conveying to the mind the impressions of sense, has vehemently resisted such an appropriation, and endeavoured to obstruct his adversary's progress with five objections. First, removal of parts of the brain is not a good mode of determining their special functions. Secondly, animals do not survive such injuries long enough to show the true alterations which have taken place in their faculties. Thirdly, one part of the brain cannot be removed without injuring others, and the effects of the injury are propagated beyond its precise seat. Fourthly, when he repeated Flourens' experiments, he did not obtain the alleged results (*Sur les Fonc. du Cerv.* III. 379.) To these objections Coster, the pupil of Rolando, replies; First, that removal of parts of the brain is the very best mode of determining their special functions, and is essentially nothing else than an example of true experimental analysis; Secondly, that Rolando's birds and quadrupeds survived the loss of the brain or cerebellum twenty-four or thirty-six hours, and reptiles several months; Thirdly, that the fallacy, arising from the injury propagating itself to the adjacent parts, may be obviated, and has been obviated, by observing the effect of the reciprocal destruction of these parts; Fourthly, that M. Gall should not venture to uphold his few, meagre, imperfect, and most prejudiced experiments, before the careful and varied researches of Flourens and Rolando, and still more the deliberate approval of the illustrious Cuvier and his no less distinguished coadjutors (*Archives*, I. 413.) Gall's fifth objection is a very odd one, for such a fanciful and reckless theorist to think of; namely, the opinion, that the cerebellum is the regulator of the locomotive movement, must be held rather as a singular idea than as a true discovery. In the present state of matters, it appears to us no small proof of the validity of Flourens' doctrines, that so acute and so captious a controversialist, on a point so injurious to his system, has made so weak an assault, and has been reduced to such sorry subterfuges.

We shall now leave the discoveries and opinions of Flourens to the test of future observation, and turn to the inquiries of M. Magendie and M. Fodéra into the distribution of the nervous properties throughout the different parts of the spinal cord, and the nerves which arise from it. The discoveries of Magendie are not less important or less extraordinary, than those we have hitherto been considering. For he thinks he has made out, that the posterior twigs of the nerves and the posterior cords of the spine are intended for conveying sensation only, while the anterior roots of the nerves and the corresponding cords of the spine only transmit the impressions which call forth muscular contractions.

Actuated, he assures us, by no preconceived ideas, but by pure unbiassed curiosity, he succeeded, after many failures, in dividing, within the vertebra *theca* of a young dog, the posterior roots of the nerves which supplied one hind-leg; and was not a little astonished to find, that the sensibility of that limb had been completely destroyed, while its motions remained unaffected. After repeating this experiment several times, and uniformly obtaining the same results, he naturally thought next of trying the effect of dividing the anterior roots only. Many obstacles prevented the accomplishment of this operation. But at last his attempts were crowned with success; and he was gratified with finding, that now the limb was paralyzed, while its sensibility was unimpaired. The same set of experiments was tried, with the same results, upon the anterior and posterior roots of the nerves of the fore-leg (*Journ. de Physiol.* II. 276.) Analogous effects were afterwards witnessed on pinching or pricking these different roots. Yet the latter test rather favoured the idea, that the fasciculi for motion had also a slight power of conveying sensible impressions, and those for sensation a slight power of exciting muscular contraction; and this idea was farther confirmed by the effects of galvanism. For when the anterior fasciculi were pinched, pricked or galvanized, besides violent contractions, slight sensation was produced; and the contrary took place when they were applied to the posterior fasciculi. The most remarkable proof of the anterior fasciculi being alone intended for exciting motion, was afforded by dividing them in an animal under the influence of nux vomica. This deadly poison, as every one knows, excites most violent convulsions. When the posterior roots of the nerves of a limb have been divided, that limb, like the rest of the body, is nevertheless violently convulsed; but if the anterior roots have alone been divided, it remains at repose amidst the general agitation (*Ibid.* 336.) From these diversified experiments, therefore, Magendie considers himself warranted to infer, that the posterior roots of the spinal nerves are intended (at least especially) to convey sensible impressions, and the anterior ones to convey the exciting cause of muscular contraction.

In a subsequent inquiry, he has extended these doctrines to the parts of the spinal cord corresponding with the anterior and posterior nervous roots. That is, when he pricked the spine between the posterior roots, he excited sensation only; when he did the same to the anterior surface, he merely excited motion. He farther remarked, that the Impressibility of the spinal cord was much more delicate than that of the nervous roots, which have their origin in it; the pain and contractions produced by pricking it being much more violent. But the central part is scarcely impressible at all; a stilet may be thrust along the centre of the whole spine, without impairing either sensation or motion. (*Ibid.* III. 153.)

These observations were scarcely published, when the merit of having made the fundamental part of them thirteen years before was claimed by Mr Charles Bell of London. In a pamphlet, circulated only among his private friends, and entitled, *Idea of a New Anatomy of the Brain*, that gentleman remarks, "On laying bare the roots of the spinal nerves, I found that I could cut across the fasciculus of nerves which took its origin from the posterior portion of the spinal marrow, without convulsing the muscles of the back; but, on touching the anterior fasciculus with the point of the knife, the muscles of the back were immediately convulsed." p. 22. This is certainly Magendie's very experiment. But Mr Bell does not seem to have drawn any positive or general inference from it; neither has he attached to it the importance he would have done, had he been aware of its tendency; otherwise he would hardly have confined the knowledge of it to the narrow circle of his private friends. Yet Mr Bell does not seem to have entirely lost sight of his original experiments; for in a communication addressed to Dr Cooke, and published by that gentleman in his work on Palsy, he observes, " The nerves of sensation and motion are bound together in the same membranes, for the convenience of distribution; but there is reason to conclude, that they are distinct throughout their whole course, and as distinct at their origin in the brain, as in their final distribution to the skin and muscles." †

We apprehend, therefore, that if the opinion as to the function of the different parts of the spine and nerves prove correct, although the honour of perfecting it belongs exclusively to M. Magendie, yet the merit of having first made it must be surrendered by him, at least in part, to Mr C. Bell. And it is with pleasure we observe, that in a late Number of his Journal, the French physiologist has done ample justice to the claims of our countryman.

But, of course, it is far too singular and too important a discovery, to be admitted without farther and repeated confirmation by other physiologists. The only other person who has hitherto published any experiments on the subject is M. Fodéra. He has sufficiently confirmed the doctrine, so far as concerns the roots of the nerves. But his experiments on the spine itself have been left by him in so incomplete a state, that it is scarcely possible to apply them with precision to any purpose. Yet they are probably sufficient to show, that Magendie's opinion on the difference of properties betwixt the anterior and posterior cords, must receive some modification. Thus he found, that at the middle of the back, the division of the two anterior cords of the spine destroyed the sensibility of the hindlegs, and not their motion, as we should have predicted; while a section of the middle of its posterior surface, for two-thirds of its breadth, did not diminish their sensibility, but impaired their motion. Again, when the right posterior cord was divided in the region of the first lumbar vertebra, the sensibility of the right hindleg was diminished, while the power of motion continued; just as Magendie's doctrine led him to expect. But then, the power of motion was diminished in the left hindleg, while its sensibility was unimpaired. On the contrary, the very same effect was produced by an incision between the second and third cervical vertebræ, not of the right, but of the left side of the cord. It would appear, therefore, that somewhere between the cervical and lumbar region, the four spinal cords cross each other, or somehow make an interchange of properties. But, as has already been hinted, Fodéra's experiments present too many gaps to permit the establishment of these or any other inferences.

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† Cooke on Nervous Diseases, vol. II. Part I. 1821.

Relative to the nervous properties of the spinal cord, M. Fodéra has observed two other facts, worthy of notice here, although, on account of the imperfections in his researches, not yet susceptible of an exact explanation. The first of these is, that after sensibility or the power of motion has been impaired, by dividing a part of the cord, it sometimes returns in a few hours almost to its pristine vigour. The second is, that in very young animals, the complete severing of the cord does not altogether paralyze the muscles posteriorly; they are no longer, indeed, under the influence of the will; yet the limbs execute combined movements with accuracy. He even adds, that they are sensible. But we presume that this is in error in terms; for he surely cannot mean that impressions on these limbs excite true sensation. It is worthy of remark, that the complete destruction of a portion of the spine, even in the youngest animals, causes complete and irretrievable palsy.

The remainder of this memoir is chiefly occupied with some interesting and ingenious remarks on the functions of the fifth cerebral nerve, and portio dura of the seventh. As they form but a part of his researches, the sequel of which is promised in Magendie's next Number, we shall pass them at present very cursorily. M. Fodéra does not wholly agree with the well-known ideas of Mr Charles Bell regarding the functions of the portio dura of the seventh cerebral nerve. For he found, that, in rabbits, the division of its branch, which supplies the muscles of the ear, takes away sensibility as well as the power of motion from every part supplied by it; and when it is pricked, the animal expresses pain distinctly. He has found, that the whole of the fifth cerebral nerve is intended for transmitting sensible impressions only. This had been already proved by Mr Charles Bell, with respect to the infra-orbital branch; and a general idea also prevailed, that the lingual portion was similarly circumstanced. But Fodéra has established it experimentally in regard to the whole nerve. When this nerve was divided just where it enters the skull, an operation of great nicety and difficulty, the sensibility of the tongue, palate, cheek, nostrils and eye-lids, was altogether destroyed, while the motion of all these parts remained entire or nearly so. As to its lingual branch he observed, that the section of it on both sides rendered the tongue completely insensible, but did not impair its movements; while the section of the hypoglossal or ninth cerebral nerve at once paralyzed it completely.

Having concluded these long and intricate details, it only remains for us, farther, to give a short account of a new hypothesis advanced by Professor Rolando, respecting the nature of the nervous influence. It is professedly derived from his experimental discoveries already mentioned, and is established, if we credit his pupil M. Coster, on the most solid and immoveable foundation. The cerebellum, according to Rolando, is a voltaic pile, which secretes a fluid analogous, if not identical with galvanic electricity; the medulla oblongata is the conductor, in which the fluid is accumulated; and the spine and nerves the channels, through which it is conveyed to the muscles for the purpose of exciting voluntary motion. But the retrograde impressions of sense do not arise from any motion in this fluid; they are conveyed from the circumference to the centre of the nervous system by a species of vibration. This hypothesis, like many others, acquires a fallacious stability by explaining neatly some curious facts in the nervous operations. It shows, for example, why a divided nerve may soon begin again to convey sensible impressions, while it regains but slowly, or not at all, its power of exciting muscular contractions; because the galvanic fluid is transmitted much more easily, than any mechanical vibration, along an interrupted conductor. On the other hand, it shows how sensation may return, and not motion, because the nerve, in healing, may have had its deficiency filled up by a matter solid enough to transmit vibrations, although a non-conductor of the galvanic fluid. Such he considers to be precisely the purpose of the ganglions on the great sympathetic nerve. A ganglion he assumes to be a non-conducting substance, interposed here and there to prevent the passage of the fluid which excites voluntary motion, while from its firmness, it does not impede the progress of sensible vibrations. The hypothesis,—for we will not abuse the language of science by calling it a theory,—is pretty enough and probably just as true as any that has gone before it into oblivion; yet a silly production withal for an illustrious professor to engender, and but a clumsy bauble for his pupil to make such a noise about. Specious, like most hypotheses, in its superstructure, and pleasing in its applications, it is not the less absurd in essence, and false in its fundamental principles; and it merely adds another instance to the numerous and well-known examples of the weakness of philosophers, when they once overstep the strict limits of experimental induction. Mechanicians will doubtless be eager to learn from M. Coster or his patron what new sort of vibration, what unheard-of vibratiuncle it is, which can travel with the quickness of thought along matter of such consistence as medullary tissue. And physicians must lament with him that Nature, when she made the complicated structure of the nerves, had not rather chosen for the purpose as many metallic threads; which would have conveyed his vibrations and galvanic fluid so much more expeditiously, and admitted of such ready repair whenever they went out of order.

In the foregoing summary we have done little more than merely allude to the experiments of Mr Bell and M. Fodéra on the functions and properties of the individual nerves. We have abstained from minuter inquiry, because the opinions of both of them must probably receive considerable modification from future experiment, and because many new discoveries remain to be made on the subject by the numerous physiologists at present occupied with experimental researches on the nervous system; so that, by a little delay, we have no doubt we shall ere long be able to present our readers with a much more interesting, precise, and stable body of doctrine, than could possibly be drawn up now.

III.

1. *Histoire des Phlegmasies ou Inflammations Chroniques, fondée sur de Nouvelles Observations de Clinique et d'Anatomie Pathologique: Ouvrage Présentant un Tableau Raisonné des Variétés et des Combinaisons diverses de ces Maladies, avec leurs différentes Méthodes de Traitement.* Par F.-J.-V. BROUSSAIS, Docteur en Médecine de l'Ecole de Paris, Médecin des Armées, Membre Correspondant de la Société Médicale d'Emulation de Paris. Vol. I. comprehending Chronic Inflammations of the Lungs and Consumption. A Paris, Gabon. 1808.
2. *Uebersicht der neueren Fortschritte in der Lehre von den Lungenkrankheiten.* Vom HERRN DR ROMBERG, praktischem Arzte zu Berlin. Fortsetzung.

View of the recent Progress of the Literature of Pulmonary Diseases. By Dr Romberg, Practical Physician in Berlin, From the Number of Horn's Archives of Medical Experience for July and August 1822, p. 55.

The recent literary history of Consumption presents several interesting points, attention to which may be instructive not only to the rising members of the profession, but also to many who have been long concerned in its practice. The difference of pathological principles, the complete opposition not unfrequently displayed in modes of treatment, and the occasional happy results of empirical measures, may be said now to admit of more satisfactory explanation, since the nature of the disease has been more thoroughly investigated. It is the peculiar glory of modern physic, that she has availed herself of the aid of pathological anatomy to improve the art of healing, and to furnish the physician with the most direct and energetic means of controlling disease; but it must be acknowledged, that the treatment of consumption has derived but little advantage from this source; and we fear that inquiry would show this malady to be as rarely cured at the present moment, when pathology is zealously cultivated and well understood, as in former periods, when it consisted of fiction and hypothesis, and when treatment was too often entirely empirical. It is indeed to be regretted, that the unsuccessful results of treatment suggested by reason and principle, furnish a strong pretext for adopting the bold and blind measures of empiricism; for when rules of science fail, it may be said, can the practitioner be censured for availing himself of those resources, the efficacy of which is demonstrated by experience? This specious argument, we regret to say, has too often been resorted to as a principle of action; and the history of physic betrays too many examples of the fluctuating and contradictory system employed in the treatment of consumptive disorders. It must not be concealed, that the writings of physicians record instances in which persons labouring under complaints declared by experienced judges to be unequivocal symptoms of consumption, have yet recovered from the most hopeless condition, and enjoyed a considerable degree of health for many years, sometimes for the duration of a long life. It is equally necessary to acknowledge, that cases of reputed consumption are daily occurring, which the rules of art fail to control, and which, however, appear to yield to modes of treatment sometimes extremely simple, sometimes completely opposite to that recommended by physicians. The ordinary manner of setting aside the evidence of these reputed cures or recoveries, is by denying the identity of the disease and true consumption, and thus stigmatising the medical adviser with the ignominious distinction of being unable to recognise the difference. We are unwilling to admit a conclusion so invidious to those whose interest it is to maintain that they succeeded in curing consumption; nor do we regard it as altogether just, that those who have been unsuccessful should, without inquiring into circumstances, deny the authority of facts established on the testimony of those who must be regarded as competent witnesses. A more liberal and not less philosophical construction would suggest, that there are only three modes in which facts of this description can be explained. Either the instances thus recorded were examples of disease which simulate genuine consumption, or the methods employed were really efficacious, or the disease is spontaneously curable. If we admit the first of these positions, it follows, that our present semeiography of consumption is inaccurate; or, if this be denied, that the same train of exterior signs depends on very different pathological causes. That the methods of treatment were uniformly efficacious, cannot be believed, when they are found so generally unsuccessful in the hands of other physicians than those who originally introduced them; for it is quite inconceivable, though allowance be made for the usual uncertainties in the quality of remedies and idiosyncrasies of patients, that all trials whatever, when conducted with requisite precautions, and with due regard to circumstances, should be abortive. The third alternative, therefore, is the only one which remains; and if it be admitted that the semeiography is correct, and remedies inefficacious, it follows, that consumption occasionally admits of a spontaneous cure,—a conclusion so contrary to every thing that experience has met with, or theoretical considerations teach us to expect, that it is utterly impossible to admit it without further and more positive examination. Yet this conclusion, however absurd it may appear, is not only implied in the evidence of such cases as those to which we have alluded, but forms also the basis of every method of treatment, and every antiphthisical remedy with which modern practice at present abounds, or may afterwards be enriched; and their efficacy or inutility must be estimated according as the sanability of consumption shall be established or disproved on satisfactory grounds. The progress of science, therefore, and the interests of society require, that all who are concerned in this great professional question, should investigate the facts and arguments on which its decision depends; and, by inquiring into the pathological character of the disease or diseases referred to the head of consumption, ascertain how far they may be regarded as capable of being cured. This inquiry is undoubtedly embarrassed with many contradictory facts and incredible results, which it may be difficult to reconcile, or explain in a satisfactory manner; but we are not without hopes, that, by contrasting the nosological characters with the pathological causes of the disease, we shall be enabled to discover whether they furnish evidence of the sanability of consumption, and of the process by which cure or recovery is effected. In the course of this inquiry, we avoid details of phenomena and symptoms, as we presume that these are superseded by the writings of Broussais, Armstrong, Abercromby, Hastings and Laennec; and strictly confine our present observations to those pathological points which contribute to illustrate the main subject of investigation,—the extent to which diseases of this description are naturally manageable, or under the control of the physician.

The nosological character of the disease termed Consumption, Decline, or Wasting (phthisis), is derived from the obvious symptoms which the person of the patient presents; and if we adopt, as an average example, the enumeration of phenomena given by Cullen, we find, first, that they are liable to variation, and, secondly, that they do not always depend on the same pathological cause. Of the five circumstances, for example, chosen by this physician as characters of consumption, * three at least, viz. wasting, weakness, and constant fever, are common to every disease in which there is local disorganization, or a process of destruction accompanied with chronic inflammation; and they may be both considerable and conspicuous, while the tissue of the lungs is neither tuberculated, destroyed by ulceration, nor otherwise diseased. Cough and exscreation of purulent matter only are distinguishing characters; and if we are allowed, according to the principles of fair reasoning, to object, that the nosologist himself, in defining the incipient disease, has admitted that one of them is not essential, and is not always present even in its confirmed form, we arrive at the conclusion, that cough, with occasional difficult breathing, is the only pathognomonic sign by which consumption can be recognised. As it is certain that this last symptom does not uniformly depend on the pathological process of genuine consumption, the practical physician is reduced to the conclusion, that there are no positive means of recognising consumption before expectoration has taken place; that when this has occurred, it may arise from other causes; and consequently, if his observation is casual, or confined to particular periods only of the disease, its nature may remain completely unknown. It may also be observed, that it is well known to those familiar with the phenomena of consumptive cases, that examples are not unfrequent, in which all the other symptoms appear, and continue till the fatal ter-

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* Corporis emaciatio et debilitas, cum tussi, febre hectica et plerumque expectoratione purulenta. Synopsis Nosol. G. XXXVI.

mination, though no discharge of purulent or tubercular matter had ever occurred; yet examination showed the lungs occupied with tubercles in various degrees of maturity, and other changes in structure. On the other hand, it has been ascertained by many observations, but especially by those of Laennec, that the destructive process which constitutes genuine consumption may be considerably advanced, without giving rise to the fever, weakness and wasting, which are uniformly ascribed to it.† It is chiefly to circumstances of this description that the difficulty of ascertaining the effects of medical treatment, and powers of remedies in the management of such disorders is to be imputed; and, if we add to this the consideration, that in the cases reported to be cured, we can rarely obtain that evidence, without which reasoning must be conjectural, and assertion groundless, it must be admitted, that unless the pathological process of genuine consumption can be more exactly distinguished during life, it is impossible to determine whether all the cases in which remedies have been exhibited with benefit, have been actually examples of consumptive disease. It may indeed be urged, that these difficulties or fallacies are to be ascribed to the imperfect semeiography hitherto adopted, and to deficiency of pathognomonic characters; and that we ought therefore at once to renounce all inquiry or examination of evidence, where the elements are so little tangible. Though this argument is so rational, that we shall subsequently give it more attention, and attempt to show its practical importance, consistency requires us to adhere, in the mean time, to that system of semeiography on which practitioners and experimental physicians have relied, in forming an opinion on the state of the lungs, and in estimating the effects of remedies, and of modes of treatment.

It appears, therefore, that the assemblage of morbid phenomena, at present called *symptoms of consumption* by physicians, may arise from pathological causes, or processes, which ought to be distinguished from each other. Though these causes, or pathological processes, will be so much more numerous, according as the diseases compared are examined in the early or more advanced stages of their progress, we shall, however, confine our attention to those morbid states only which are liable to be confounded, and in which the symptoms, according to the ordinary practical doctrines, suggest the use of those measures which are conceived to possess some antiphthisical or sanative power. Proceeding on these principles, we find, that the external signs by which physicians are in the habit of recognising the presence of consumption, may depend, 1*st*, on chronic inflammation, or other morbid conditions of the bronchial membrane; 2*d*, on ulceration, with chronic inflammation of the larynx or trachea; 3*d*, on chronic inflammation of the pleura; 4*th*, on inflammation with suppuration of the lung; 5*th*, on tubercular destruction of this organ.

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† A Treatise on Diseases of the Chest, &c. by R. T. H. Laennec, p. 42, 303, translated by Dr Forbes.

I. The pulmonary mucous membrane is liable to inflammation in various degrees of severity, from the slightest cold or catarrhal affection, to the most violent peripneumony; and as the action is rapid and transitory, or slow and more lasting, it is said to be acute, subacute, or chronic. In this condition, dissections inform us, that a space more or less considerable of this membrane becomes redder than natural; and what was previously uniformly grey and smooth, appears to be traversed by minute vessels, or dotted with numerous reddish asteroid points. At the same time, the membrane becomes softened, villous or rough, pulpy, † and thicker than natural, or more swelled, so that the calibre of the smaller bronchial tubes, and the capacity of the pulmonary vesicles, is considerably diminished. These appearances, which may be said to constitute the anatomical characters of simple inflammation of the bronchial membrane (bronchitis), are common both to the acute and chronic forms of the disease. In the latter, however, it appears that they take place much more slowly than in the former, and the membrane, though reddened and villous, is rarely so much thickened, or rather swelled, as in the acute disease.

A uniform effect of this action in the pulmonary mucous membrane is, to augment the quantity, and change the quality of that fluid, which it secretes in the natural state; and there is reason to believe, that every derangement, however moderate, of this secretion, depends on some form or degree of inflammation. The extent to which these changes take place, varies according to the severity and duration of the disease; and this gives rise to considerable variety in the characters of the fluids discharged by expectoration. At the commencement of chronic, or even acute inflammation of the bronchial membrane, the bluish, semitransparent, and particled mucus of health is mingled with mucilaginous, transparent, and greyish fluid, not unlike white of egg, which is secreted in considerable quantity. As the morbid action goes on, however, it becomes thicker, more viscid and opaque, and generally sinks in water; and, when fully established, this thickened mucus is either mingled with, or entirely converted into, a yellowish, opaque fluid, which cannot be distinguished from purulent matter, and which is generally more or less streaked with blood. This is the ordinary change of the secretion of the bronchial membrane when inflamed; but variations are often met with. The matter expectorated may be merely thickened mucus, very opaque, and condensed; or it may be mucus much streaked and mingled with blood.

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† Hastings, 281.

It might be supposed that these changes could not be effected without breach of continuity, or ulceration of the membrane; yet it is established on the most unquestionable evidence, not only that purulent fluid may be expectorated, but that all the concomitant signs of consumption may occur without other change of the condition of the lungs, than the process of inflammation of the mucous membrane. In the course of his practice at the Hospital of Vienna, De Haen ascertained, on examining the bodies of persons who, after copious expectoration of puriform or purulent matter, had died with the usual symptoms of consumption, that no ulcerated breach could be detected, either in the lungs or bronchial membrane. * The same fact was established by Dr Willan, in many cases of this disease which fell under his observation in the spring of the years 1796 and 1798; † and by Dr Badham, who has shown, in his short Treatise on Bronchial Inflammation, that this disease gives rise to all the phenomena of consumption, and may eventually disappear. ‡ More recently, Dr George Pearson, who has examined attentively the chemical characters of the various kinds of expectorated matter, has shown, that an opaque, white, or yellow fluid, equally consistent, but more tenacious than cream, is discharged from the pulmonary membrane, without breach of surface, in two different conditions of the organ. Firstly, in the instance of a woman, who, in the third week of an attack of measles, expectorated more than a pint of green-

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* Rationis Medendi, I. XI. p. 60.

† " The disorder had not, therefore, in any case, produced ulceration of the lungs; and the expectorated fluid, so alarming in its appearance, was perhaps only composed of a puriform secretion and an increased discharge of mucus; circumstances usual in other membranous inflammations. It is worthy of remark, that by such a state of the inner surface of the lungs, the constitution is affected nearly in the same manner as when they are ulcerated; on which account, the diagnosis of pulmonary consumption must often be rendered obscure and difficult."—*Willan's Reports*, 1796, 20*th March*.

‡ Observations on the Inflammatory Affections of the Mucous Membrane of the Bronchiæ, by Charles Badham, M.D. &c. &c. pp. 48. 76.

ish cream-like matter every twenty-four hours,—after death, which took place in a few days, careful examination of the lungs disclosed neither ulceration of the bronchial membrane, nor tubercles or abscesses of the pulmonic tissue. Secondly, in the case of a man who expectorated a fluid considered purulent by all who saw it, and believed to proceed from ulceration or tubercular softening, it was found, that the only morbid change was condensation or consolidation of the lungs, with watery effusion in the cavity of the pleurae. † In cases similar to the first of these, which are now known to be exceedingly common, the puriform or cream-like fluid is secreted by the membrane in a state of chronic inflammation; in those of the second sort, which, as will appear, are equally common, the membrane is either inflamed, or becomes the seat of an augmented action, which depends on any cause that resists the free transmission of blood, and the healthy motions of the lungs. According to the further observations and researches of this physician, which have every appearance of being accurate, the fluid secreted from the inflamed pulmonary membrane, may present almost every variety of colour and consistence, from thickened mucus to distinct purulent fluid, or these two substances may be combined in every proportion. The general correctness of this conclusion has been confirmed by the cases of Dr Hastings, and by the daily observation of many examples of purulent expectoration.

The frequency with which a greater or less proportion of blood is mingled with the matter exscreated in chronic bronchial inflammation, requires that some notice be bestowed on this phenomenon. The discharge of blood by expectoration, has been too often regarded as a disease, when it ought to have been considered as a mere symptom. In one condition only of the lung, can it be with any reason considered as an individual disease; and even here it might be shown to be the result of a preliminary process, consisting of local vascular congestion. In all other circumstances, however, it is uniformly the effect, and consequently the symptom, of a morbid process. We allude to those cases in which blood is expectorated in small quantity, almost pure and fluid, or mingled with more or less muco-purulent matter. This is the *haemoptoe* described by nosological writers, and justly distinguished from *haemoptysis*. It occurs so frequently in chronic bronchial inflammation, that it may almost be regarded as a symptom of the disease; and instances occur in which the only symptom is the occasional exscreation of blood, or bloody mucus, for a long time, sometimes with intervals of health for years.

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† Transactions of the Royal Society, 1809, Part II. p. 315-321. On the Qualities of Expectorated Matter, by George Pearson, M.D.

In these circumstances, the blood, whether pure or mingled with mucous or purulent fluid, is discharged from the bronchial membrane without destruction of tissue or rupture of vessels, or, in the language of some physiologists, is exhaled. "I have often opened," says Bichat, "persons who have died during hemorrhage, and have examined the bronchial, gastric, intestinal, and uterine surfaces, yet have not perceived the slightest trace of erosion, notwithstanding the precaution of washing them with care, allowing them to macerate, and afterwards submitting them to examination by means of a lens."‡—"The hemorrhages also," he remarks, "which are occasioned by violence, and produced by rupture, viz. from the nose and ears in injuries of the head, from the bowels and urethra in other injuries, have phenomena and duration completely different from those which take place from a mucous surface." Many other facts show, that blood, even in considerable quantity, may be discharged by expectoration, without rupture or ulceration of the pulmonary membrane. It is to be regarded, then, as a symptom of inflammation, appearing when this is present, and vanishing when it subsides.

The duration of this disease is various, according to circumstances of constitution, treatment, and external agents in general. It is seldom shorter than three or four weeks, and it may extend to sixty days, or three or four months with considerable change in the violence of its action and effects on the constitution. In general, however, before this time it manifests a tendency either to subside, to affect the submucous tissue of the lung, or it may terminate the existence of the patient, by the violence of its constitutional effects. That it may terminate spontaneously, especially if favoured by the cooperation of mild weather, and shelter from exciting causes, we have the united testimony of many authors; but more especially of Dr Badham, Willan, Broussais, and Hastings; and recovery is more certainly effected under the use of those means which are known to control inflammation.

"In several patients," says Dr Willan, "most of them females, a hard sounding cough was, after some days, followed by an expectoration of thick fetid greenish matter, intermixed with a large quantity of clear viscid mucus. Along with this, other symptoms appeared, which seemed to indicate the appearance of *phthisis pulmonalis*, as diarrhœa, hectic fever, night sweats, and emaciation. The complaint was much aggravated during the frost, which began in the last week of February, and terminated on the 11th March. It was not at all alleviated by blood-letting employed at an early period of the cough. The most effectual remedies appeared to be cupping between the shoulders, blisters applied to the sternum, and *scrobiculus cordis*, pediluvium, antimonials occasionally, nitre and demulcent drinks. Under this treatment, added to a light cooling diet, all the above patients were restored to health before the 20th March." And afterwards, in his Report for the spring quarter of 1798, he states, that "many persons who had catarrhal coughs in March, were farther affected with spitting of blood, thick viscid expectoration, pains within the chest, hectic fever and diarrhœa, interchanging with night sweats, but recovered notwithstanding in the month of April." This observation he afterwards remarks, enforces one practical point of some consequence, "that in coughs succeeding to catarrhal fevers, we should not be too hasty in pronouncing a case to be decidedly phthisical; nor, whenever the lungs are inflamed, discontinue, so long as a possibility of benefit remains, the proper regimen and the means of resolution."

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‡ Anat. Generale, Vol. I. p. 563—565.

This disease, which constitutes what has been called *catarrhal or pituitous consumption*, has too frequently been distinguished by practical authors, into as many varieties as the exciting causes which are concerned in its formation; but as this subdivision is at once irrational and of no practical use, it ought to be abandoned. Whether the disease succeeds to acute inflammation, or catarrh, or measles, or hemoptysis, or irritation, mechanical or chemical, or the deranged condition of dyspepsia or liver disease, or the morbid susceptibility of fever, or accompanies other morbid actions, as spitting of blood and peripneumony, it is uniformly to be regarded as the same pathological process,—as inflammation more or less violent of the pulmonary mucous membrane, and as effecting the usual derangement in the functions of this membrane. That form of it which depends on disorder of the chylopoietic viscera, described by Mr Abernethy and Dr Wilson Philip, though occasioned by a peculiar remote cause, is pathologically the same morbid action as the chronic inflammation which succeeds to measles or pneumonic disease.

The testimony of De Haen, Stoll, and Frank, show that it forms a great proportion of consumptive cases in Germany; in Russia it appears to be frequent; it is noticed by Dr Lionel Chalmers as particularly prevalent in Carolina, and by Dr Rush in North America in general; and there is reason to believe, that the rapid cases which occur among seamen in the Mediterranean and West Indies, consist originally of this disease. It is indeed by far the most frequent of pulmonary com-

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† Reports, 20th March 1796.

plaints, and it may be considered as certain, that, in the greater number of consumptive cases, chronic inflammation of the bronchial membrane is either the principal, or the only pathological action. It is indeed true, that it generally occasions inflammation and induration of the pulmonic tissue, before it terminates fatally; and it might be argued, that according to the evidence of morbid dissections, the disease should be regarded as chronic peripneumony; but as we shall show that this is the result of the previous inflammation of the mucous membrane, we conceive it most proper to refer the disease to this latter head. There is reason to believe, that the greater number of cases of reputed consumption occurring in large towns, and marked as such in the bills of mortality, are examples of chronic catarrh, with or without pulmonary consolidation; for the generality of tubercular disease, though very great, has been much overrated. The only existing evidence for the prevalence of true tubercular consumption, depends on the most questionable testimony; and as we cannot justly admit, without actual dissection, the presence of tubercles in every case said to die of consumption, it is obvious that the means of ascertaining the point should be more positive. Judging from what we see in this city, we should believe that a small proportion indeed of cases with consumptive symptoms are to be referred to tubercular disorder; and we find the testimony of Dr Willan as to London leading to a similar conclusion.

" In my own list, " says the physician, " the article of pulmonary consumption includes cases of ulcerations of the lungs, and alterations of their texture, in consequence of pneumonic inflammation, and repeated catarrhs. I apprehend not more than *a fourth part* of the whole number of cases put down could be referred to proper phthisis, arising from the *slow and successive suppurations of tubercles* in strumous constitutions." Reports, 1797. April.

The proofs which we have here adduced may be sufficient to establish the two leading facts in this inquiry; 1. That chronic inflammation, without ulceration of the bronchial membrane, will give rise to the constitutional symptoms of consumption; and, 2. That this disease, in favourable circumstances, is susceptible of cure spontaneous, or effected by art; and it is therefore superfluous to occupy more time in establishing what must be already obvious. We cannot however quit this part of our subject, without remarking, that this view affords a satisfactory explanation of the apparent efficacy not only of reputed remedies, but of recoveries from consumption, which have been reported to have occurred. Various cases of this kind are recorded in the writings of Darwin, Beddoes, Kinglake, and Magennis, and we think perusal of these cases will show, that the greater number, we may almost say the whole, of those which recovered were examples of chronic bronchial inflammation. The five successful cases which are particularly described by Dr Magennis, are evidently to be referred to this head; † for the symptoms, at least as far as a judgment can be formed, and the effects of the remedy, indicate an inflammatory action only. The instances of cure, related by Dr Beddoes, took place under considerable variety of treatment. Three were effected by stabling with cows; two by regulated temperature; and two by combination of foxglove and antiphlogistic remedies. The case of Dr Briggs is evidently an example of pulmonary apoplexy, with local inflammation of the bronchial membrane. Of four other cases published subsequently by this author, the symptoms and mode of formation indicate nothing like tubercular destruction, and we think there can be little doubt that they were distinct examples of chronic inflammation of the bronchial membrane. This opinion is founded on the following circumstances; 1. The cases to which we allude commenced, not with the usual latent and imperceptible disorder which is known to attend tubercular disorganization, but with more or less of the ordinary symptoms of cold, and a greater or less degree of the rapidity which is known to mark the accession of inflammatory disorders; 2. Their symptoms were of the sort that may be referred to mere inflammation, unaccompanied with tubercular deposition; 3. In the process of recovery no similarity can be traced to that which marks the recovery of distinct and unequivocal cases of tuberculated lung. We know, from the observations of Laennec and some others, that in a few cases the tubercular matter may be eliminated, and recovery may take place. But in the course of this process, which is tedious, none of the symptoms give way so rapidly as they did in the cases to which we allude; and after convalescence, the patients were always more or less liable to be influenced by any slight affection of the chest. ‡

We know not if we may venture to offer the same explanation of cures of consumptive symptoms treated on a different plan, and terminating favourably; and it is not unlikely, that we may cause many to think, that in our eagerness to explain, consistently with pathological principles, the success of the most opposite methods, we may destroy in some measure the strength of the evidence which we have already adduced. We allude particularly to those cases which are said to recover under what is termed the tonic treatment. To pass over the few examples recorded by May, the following may be regarded as the most imposing evidence. In the year 1801 Charles Pears, a surgeon in the neighbourhood of London, published reports of forty-nine cases termed pulmonary consumption (*phthisis pulmonalis*), treated according to what he terms the tonic plan; that is, with a liberal allowance of animal food, wine &c., and with medicines supposed to possess a strengthening or stimulating power. * In his prescriptions, which are given at full length, gentian, either in infusion or powder, is a uniform ingredient, and valerian, nitre, spirits of nitrous ether, or opium, are frequent adjuncts. Bleeding he never employed, and he deprecates its use in the strongest terms; but a blister appears occasionally in his prescriptions, and ointment of tartrate of antimony was employed once. † Under this treatment, twenty-one out of forty-nine cases were permanently restored (cured); eighteen refused to comply with treatment, which is very extraordinary; and only ten died. Of these last, the details of three only are given; in two of which the fatal event is ascribed to the intemperate and habitual use of spirits, and in the third to exposure to cold during treatment. Not much unlike is a species of treatment, which has been long pursued by a celebrated extraprofessional gentleman, and of which notice is now occasionally taken in the Journals. ‡ Of these cases the particulars are not well known; but until more decided proof is adduced, that they depended on tubercular destruction, we think it is but fair to refer them to that form of disordered action which depends on bronchial inflammation.

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† Medical and Physical Journal, Vol. XXV.

‡ On Diseases of the Chest, p. 51.

It is not always however that this malady terminates so favourably, and that it is attended with local injury so trifling. In its mildest form, we have already seen that the pulmonary membrane is merely inflamed, and that one of the effects of this is to augment the quantity and change the quality of its mucous secretion. This process is not unfrequently attended with more serious morbid changes; and, in many cases, dissection has shown minute ulcers to be formed in the membrane. It does not appear that the process which terminates in ulceration of this membrane has been completely investigated, or that the mechanism of the formation of ulcers has been satisfactorily described. They are certainly not necessary to chronic inflammation; but it has been found, that they are most common in the persons of those whose occupation exposes them to inhalation of irritating mechanical powders. Such, for example, dissection has shown to be the state of the bronchial membrane in stone-cutters, ‡ glass-grinders, needle-grinders, § and the leather-dressers of Worcester. ‖ In such circumstances, in which the presence of mechanical irritating substances excites inflammation, succeeded by suppuration, and more or less destruction of tissue, it may be supposed that the morbid action occasioned in this manner would subside, as soon as, by its own means, it had removed the cause of its action. The cases however of this kind which have been recorded, show, that though recovery occasionally takes place in the early stage, it is almost never effected after distinct suppuration has occurred.

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* Cases of Phthisis Pulmonalis successfully treated upon the Tonic Plan, with Introductory Observations, by Charles Pears, F. M. S. &c. &c. Lond. 1801.

† The 18th. ‡ On the Tonic Treatment of Phthisis, by Dr John Hume, in the Quarterly Journal of Foreign Medicine, &c. No. 16. January 1823.

We have less certain information on the nature of a form of ulcer considerably different, which may occur in the bronchial membrane. We allude to that in which the membrane becomes the seat of numerous minute eminences, which, as they pass through the several stages of inflammation, suppuration, and, finally, ulceration, may be regarded as pustules of the pulmonary mucous membrane. This disease is not unlike what is oftimes observed to occur in the intestinal mucous membrane, where it occasions first a modification of diarrhœa, and afterwards assumes the form of dysentery. It may be regarded as inflammation taking place simultaneously or successively, in many minute points of the membrane, and passing, after a certain time, into the suppurative stage. The ulcers thus formed are in general round or oval, rarely irregular, with their margin slightly raised, and surrounded with a red circle (areola), more or less distinct. The matter expectorated consists of purulent fluid, streaked with blood, and mingled with a considerable proportion of dense mucus.

In the cases of this disease from which our information is derived, death had taken place after the usual symptoms of consumption had subsisted for some time; and as it is uncertain whether, in cases which terminated in recovery, but the subjects of which had finally been destroyed by other maladies, these minute ulcers of the bronchial membrane had previously existed, our evidence as to the sanability of this sort of injury is incomplete and unsatisfactory. Medical men have in general concluded, that, when patients with hectic fever and puriform expectoration have recovered, the bronchial membrane has been simply inflamed chronically, without ulceration or breach in the pulmonary tissue; and we find Dr Willan actually reasoning in this manner in a passage already quoted. That the pulmonary tissue had not been injured in such cases, is probable enough; but it is clear that this is merely matter of opinion, and that even the absence of ulceration of the bronchial membrane is not proved; and the circumstances to which we have already alluded show, that evidence of this conclusion is still wanting. We do not regard it as a pathological impossibility, for ulcers of the kind which we have described to undergo the healing process; and it appears equally probable, that a slight loss of substance thus produced, may be repaired under favourable circumstances, as in the case of an individual ulcer of any part of the bronchial or tracheal membrane. Are there recorded any cases, in which the usual symptoms of bronchial inflammation had, after some time, terminated in health, and in which, after death by another disease, many years after, the pulmonary mucous membrane had exhibited unequivocal traces of cicatrized pustular ulcers, similar to those which are observed in the intestinal mucous membrane of persons who have recovered from dysentery?

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‡ Coschwitz de Spadone Hippocratis, in Haller Disputat. P. II. 47.

§ Johnstone of Wercester.

‖ Hastings, p. 281.

We trust we have shown to what extent the position is true, that chronic catarrh is not necessarily a fatal disease; and that, whether it consists merely in inflammation, with augmented and vitiated secretion of mucus, or in ulceration of the membrane, it still may admit of being controlled by art, or may, in favourable circumstances, subside spontaneously. These conclusions are in perfect accordance with strict pathological observation. Yet it is notorious, that though several cases of chronic catarrh, or catarrhal consumption, actually recover, a great proportion terminate fatally, notwithstanding every thing that skill can suggest, or that art can perform. We therefore proceed to inquire into the circumstances which render this malady so intractable, and so generally fatal; and it will be found that they are to be referred to a greater or less degree of that change in the pulmonic structure, upon which, dissection informs us, depends the disease termed, by practical writers, chronic peripneumony.

When chronic catarrh has subsisted long, the inflammatory action extends from the mucous membrane to the submucous cellular tissue, which unites the bronchial tubes and vesicles to the serous or transparent membrane of the lungs. This is the proper *cellular*, not vesicular, tissue of the organ. At first, this action produces merely redness, with vascular congestion of the submucous tissue, or what the older pathologists termed infarction of the lungs. But, as the morbid state of the blood-vessels continues or increases, lymph, or lymphous fluid, is effused into the interstices of this tissue; the part loses its natural softness and elasticity; and as the bronchial tubes and vesicles are more or less compressed by this newly deposited substance, the lung loses its sponginess and lightness, which depended on the complete permeability of its vesicles. It is found, that a lung in which this chronic inflammation of the submucous tissue has existed for some time, presents the following phenomena. 1*st*, On opening the chest and admitting the air, though there are no adhesions, the lung does not collapse at all, or does so very slightly. 2*d*, The pulmonic substance which surrounds a portion of chronically inflamed membrane, becomes harder and denser than before, and does not float completely in water. If the induration is considerable or extensive, it sinks entirely. 3*d*, It loses its elasticity and compressibility, or cannot be inflated, and no longer crepitates, as in the healthy state, but resembles a portion of solid flesh.

This change of the pulmonic tissue was early observed by pathological anatomists; but we find the first distinct examples of it in the writings of Morgagni [†] and Maximilian Stoll, [‡] the last of whom recognised it in many individuals who had laboured under chronic inflammation of the lungs. It has been ascertained, by the observation of Dr Baillie, that this change is caused by accumulation of blood in the minute vessels of the part, and effusion, or extravasation, as he terms it, of coagulable lymph in the cells of the submucous tissue. [§] This deposition obviously produces two effects on the structure in which it takes place. It unites mutually the individual fibres or threads of which the pulmonic cellular tissue consists, and, by its presence, diminishes the space originally occupied by the compressible and elastic substance of the organ. The authorities to whom we have now referred, notice this change as if it were peculiar to inflammation of the lung, acute or chronic; and, strictly speaking, it forms the pathological character of this disease. It is not, however, confined to it; for it has been ascertained by Broussais, and more lately by Hastings, that it is a common consequence or accompaniment of chronic bronchial inflammation. Since the appearance of the writings of these authors, M. Laennec has described, under the head of peripneumony, three different degrees of it, and has distinguished them, 1*st*, according as the lung is red or violet, but crepitates and discharges, when cut, a frothy blood-coloured fluid; 2*d*, as the portion of lung is destitute of crepitation, and is red and granulated interiorly, without discharge of fluid when cut, unless squeezed; 3*d*, as it is consistent and granular, its section a pale yellow, or straw-colour, and as it discharges a considerable quantity of opaque, yellowish, viscid fluid, from many points of its cut surface.

[†] De Sedibus et Causis, Epist. XX. 3. 5. 22. 24. 20. 26. 36. 42. 47. 49. XXI. 26. 11. 27.

[‡] Rationis Medendi, Pars I. 184. 202. II. 370. III. 364.

[§] Morbid Anatomy, p. 60.

It is the first of these states that is more generally observed to occur in the submucous pulmonic tissue, in the course of catarrhal consumption; for though the admission of air to the cavity of the pleura does not cause the lung to collapse, and on incision it appears firmer and more solid than natural, it nevertheless crepitates slightly, and even partially floats in water. In some examples, however, of chronic catarrh, the second degree of induration has been known to take place, and considerable portions of lung have been found, not only red, indurated, and consolidated, but granular, uncrepitating, and completely sinking in water. There is reason to believe that this extension of inflammation to the submucous pulmonic tissue, is the chief circumstance which determines the fatality and insanability of the disease. We have already seen, that chronic catarrh is not of itself necessarily fatal; and recoveries frequently take place under circumstances favourable to the abatement or disappearance of the inflammation, when merely membranous; but when it extends to the subjacent, and affects the proper pulmonic tissue, it appears to be fixed in character, and obstinate against the usual remedies. The evidence may be reduced, we believe, to the following points.

1. In cases of increased and vitiated secretion of mucous, or muco-purulent fluid from the bronchial membrane, with the ordinary constitutional effects, the symptoms have gradually subsided, and the individuals have been restored. It is to be regretted, that on this point we have only negative evidence that the pulmonic tissue was not affected; for though the individuals may have presented the usual signs of peripneumony, the diagnostic mark of permeability of lung, or respirability ascertained by stethoscopic examination, has not been given. There is, however, reason to conclude, from the absence of the rational signs, as they are named, viz. quick breathing, orthopnœa, tightness, lividness of countenance, that this tissue was not, in the instances alluded to, seriously and permanently affected.

2. In cases of chronic catarrh, with the usual constitutional symptoms, which have resisted medical treatment, and gone on to fatal termination, it has been uniformly found that more or less chronic inflammation had existed in the submucous or pulmonic tissue. The most complete collection of cases with which we are acquainted, are those recorded by Broussais in the first volume of his work on the Chronic Inflammations. Of fifteen fatal cases of chronic catarrh, recorded by this author, only two presented the lungs entirely free from induration. In the other thirteen, an extent, more or less considerable, of one or both lobes, was hardened and solidified,

and, when divided, presented a greater or less degree of this change. The truth of the same fact is satisfactorily demonstrated in the results of the fatal cases detailed by Dr Hastings in his Essay on Bronchial Inflammation. In the 1st, 2d, 5th, and 8th cases, the chronic membranous inflammation was combined with much consolidation of the lungs.

3. The next point requisite to illustrate the sanability of chronic catarrh when thus aggravated, consists in the possibility of recovery after the submucous tissue has been indurated, or at least of existence and continuance of vital processes, while induration more or less extensive has taken place. The consideration of this subject leads to some curious and not uninteresting consequences. We believe it will not require much argument to prove, that the existence of induration during life has, from a variety of causes, never been sufficiently attended to; nor has any attempt to discover what characteristic symptoms it produces been made till very recently. This may appear singular to those who know that this change is neither more nor less than what ought to give rise to the symptoms ascribed by nosological authors to the disease termed peripneumony; for it might be concluded that these symptoms ought to take place in every case of induration, and consequently to indicate the pathological change. It is however certain, that in the most correctly detailed observations of the disease, such as those given by Broussais and Hastings, peripneumonic symptoms were not observable during life; and yet after death, induration of portions of lung more or less considerable were discovered. In the case given by Dr Pearson already alluded to, the disease gave rise to distinct consumptive symptoms; and we think we have seen others exactly similar. We must therefore conclude, that the characters hitherto given by nosological authors, are insufficient to determine the presence or absence of the disease; and recourse ought therefore to be had to marks more decisive and less liable to ambiguity than these appear to be. In this difficulty there is reason to believe that stethoscopic examination furnishes the only accurate test by which the permeability or respirability of the lung can be ascertained during life; and unless the respiratory murmur is thus shown to be either much impaired or completely destroyed, it is impossible to conclude in any given case, whether the morbid effusion had disappeared or not.

We have now shown, by undoubted proofs, that chronic catarrh, when it has subsided for some time, is almost uniformly attended by induration of the pulmonic tissue, and more or less change in the structure necessary to the process of breathing.

The next point of inquiry, is to examine the pathological changes which such consolidated structure undergoes, the effects to which it gives rise, and the question whether there is any possibility of removing it by remedies.

The induration of the pulmonic tissue, which is the result of chronic inflammation, might be expected, from what is observed in analogous circumstances, to terminate in suppurative or ulcerative destruction; and several authors appear disposed to consider this as a natural and frequent occurrence. * We are compelled, however, to say, that the instances of pulmonic disease hitherto correctly recorded, give little or no countenance to this opinion; and suppurative destruction of indurated lung appears to be one of the rarest occurrences almost that can happen. The reason of this will appear very soon. The course which it pursues, we are inclined, from the collection of many cases, to consider as the following. After one portion of lung has become traversed by numerous red vessels, and is beginning to be hardened by effusion of lymph, it exercises an influence on the contiguous set of vessels, and produces more or less obstruction to the free transmission of blood through the part. The contiguous parts undergo the same change. This effect is augmented by slow and successive, but very certain steps, until a considerable portion of lung, which, in the natural state, is traversed by few and colourless capillaries, is at length crowded with a multitude of red vessels, in which blood is detained, and thus interrupts the healthy circulation of the organ. While lymph continues to be separated from the blood in these vessels, an analogous process takes place in the minute vessels of the general circulating system. Serous fluid begins to be effused in the cavity of the pleura and peritoneum, and into the cellular system in general, and the patient is cut off with the symptoms of general dropsy. So uniform is this mode of termination, that Broussais informs us he was almost invariably led to infer the existence of chronic catarrh and pulmonary induration, from observing œdematous swelling of the face or extremities, or even a slight tendency to this symptom. †

"Those patients who had bivouacked in the mountains exposed to winds and snow, recovered with great difficulty. Several adynamic and ataxic fevers underwent no inconvenience; but all the catarrhs, whether simple or complicated, returned with new severity, and several died at the hospital of Bruck in which I was acting. I was unable to collect the exact history of all these catarrhs; but having opened the bodies, I satisfied myself that all those who coughed for 1½ month or 2 months, who had several alternations of fever and apyrexia, and

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* On the Pathology of Consumptive Diseases. By John Abercrombie, M. D. Edin. Med. and Surg. Journal, Vol. xviii. p. 22. † P. 90. Phlegmasies Chroniques.

who died rapidly with slight œdema, had the lungs in that state of induration, which is termed *carnification*, or, by comparison with the liver, *hepatisation.*" p. 79. Vol. I.

In another part of his work, when speaking of cases in which the tendency to œdematous swelling led him to employ diuretic remedies, he uses the following remarkable language.

"Some of these patients having died, I hastened to examine their bodies, to discover the nature of that organism which expressed itself so indistinctly, and was astonished after five or six autopsies, to find invariably hepatised lungs. With some, the violence of the cough had prepared me for this; but there were nevertheless several in whom I saw the effects of the disease, before I suspected its existence. I then observed with all possible attention those patients whose actual condition seemed to threaten a similar termination; and I discovered 10 or 12, who, after the usual course, did not, with return of appetite, recover strength, had straw-coloured complexion, and in whom rotundity of figure not referable to corpulence, indicated a disposition to dropsy. With some of these, though by no means generally, the spleen appeared tumefied; but the most important symptom was a *dry night-cough*, of which only a few complained. To determine its real existence, I resolved to visit them at one or two in the morning, and then found that the patient, who in the morning was cheerful, and asserted that he scarcely coughed at all, had *slight heat, frequent pulse, the cheeks a little red, and a dry, sometimes very violent cough.* As I observed the progress of these chronic coughs, I now was convinced of the analogy between these patients, and those whose death had shown their lungs to be hepatised. All at once the face appeared infiltered, especially at the eyelids; the hands and feet became œdematous; rattling breathing commenced, and the patient died in agony. Others became dropsical, and in a few days acquired an enormous bulk. All of them yielded to the force of the disease, and examination showed the principal lesion to be indurated lung and inflamed pleura." pp. 105, 106. Vol. I.

The next point in this inquiry to which our attention is directed, is the question, whether hardening of pulmonary tissue is susceptible of removal, or whether, if this be not the case, it is possible for the individual to exist.

It appears that a small portion of lung may continue indurated, without necessarily destroying the individual; but in this case observation shows, that the process which gives rise to induration must be suspended or completely checked. If this process still continue, the induration extends, and at length occupies a great part, or the whole of one lung; and, in such circumstances, the usual consequences of induration rarely fail to appear.

The possibility of the removal of induration appears to depend very much on the particular degree or form in which this change has occurred. If it be merely in the first degree, as described by Laennec, we have decided proofs that it not unfrequently disappears, and that the tissue of the lung is restored. In this degree of induration, the change consists in the enlargement of the colourless vessels of the pulmonic or submucous tissue with blood, in the formation of many new vessels, and in the tardy motion or slow circulation of this fluid through these capillaries. It is not difficult to see, from the usual phenomena of morbid actions, that there is nothing in this which may not be restored to its original condition. Under favourable circumstances, the blood may gradually retire from this set of capillaries, the vessels may contract, those that have been developed may disappear, and the entire structure of the part may be restored to its original and sound state. That this is possible, must be inferred from examples of peripneumony, in which the symptoms, as known from analogous cases, indicated the incipient hardening, and which have eventually recovered. That it is possible, must be inferred from the fact, that the stethoscope has, in some cases of catarrhal or peripneumonic disease, indicated engorgement, or induration in the first degree, and has subsequently indicated the gradual return of permeability of lung, and respirability with the disappearance of the symptoms. That it has actually taken place, may be inferred, we conceive, from various cases which have lately fallen under the observation of competent judges. On this head we refer to the 16th and 17th cases recorded by Broussais. *

When hardening of lung in the second degree has taken place, resolution or restoration implies a more complex process,—the disappearance not only of the enlarged and newly developed vessels, but the absorption of effused lymph. The first of these processes, we have already shown, is possible; but whether the second actually occurs, may be difficult to prove. That it is not impossible, may be inferred from the fact, that lymph which is effused in the formation of abscess, is gradually removed after suppuration, and seems to be absorbed; and also from what is observed to happen in recent effusions of lymph, which while fluid appear capable of being absorbed. But we cannot assert, that lymph which is deposited in the interstices of the submucous pulmonic tissue, is absorbed with equal rapidity and facility. Broussais appears inclined to think that it may, and quotes cases in which, though the symptoms led him to think this effusion had occurred, restoration was effected. The authority of Laennec, so far as relates to the mere change of structure, is still more posi-

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* Phlegmasies Chroniques, Tom. i. pp. 160 and 162.

tive. For he not only concludes, from many cases, that death takes place from exhaustion or general weakness, rather than in consequence of mere organic lesion, but states his opinion, that restoration may be effected without disorganization of the pulmonic tissue, even in the third stage of the morbid process, when purulent deposition has taken place. * He has not, however, examined the means by which this process of restoration is effected, or attempted to trace the progress from the obstructed and engorged, to the healthy and free state of the pulmonary tissue. There is reason to believe, that, though possible, it is but a rare occurrence; and it is so much more difficult to render it a matter of certainty, that those cases only are examined, in which the lungs are most completely disorganized; and in those in which restoration is effected, we have no means but the stethoscope to determine the point. From instances, however, of this form of pulmonary disease, which have fallen either under our own observation or that of our friends, it appears, that a very copious discharge of purulent matter, with bloody streaks, takes place from the bronchial membrane for several days, sometimes weeks, while the effused fluids are removed from the pulmonic tissue, which becomes permeable; and if the general strength of the individual be not destroyed by the constitutional symptoms, recovery takes place at an earlier or later period.

II. The process of ulceration occasionally takes place higher up in the course of the same membrane, and gives rise, as was first distinctly proved by Morgagni, to a modification termed laryngeal or tracheal consumption. There is no doubt that this ulceration is the result of previous inflammation; but the tissue originally inflamed is not always the same. It appears, in general, to commence first in the mucous membrane; and various examples are recorded in which this process had been developed under the influence of different exciting causes. One very generally blamed for ulceration of the laryngeal mucous membrane, is the poison of syphilis; and the second volume of the Annals of Thomann contains a case by Christin, in which, though the individual denied any syphilitic taint, cicatrices of former ulcers were found on the corona glandis. † In many instances of this description, we should be inclined to ascribe it to the ordinary operation of cold and moisture, and more especially if the patient has been well dosed with mercury. Of five cases of laryngeal consumption which have fallen under our observation, we have been able to trace two with certainty, and one with probability, to this cause; and we presume, from the mode

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* On Diseases of the Chest, &c. p. 50, Forbes's edition.

† Annales Instituti Medico-Clinici Wirceburgensis redegit et Observationibus illustravit J. N. Thomann, M. & C. D., &c. Volumen ii. p. 285.

of commencement and the concomitant symptoms, that inflammation of the mucous and submucous tissue of the laryngeal membrane, acute, subacute or chronic, was the beginning of that process which merged in ulceration with the usual constitutional signs. In other instances, it appears to have originated in disease of some of the subjacent tissues; and examples have occurred in which ossification of the laryngeal cartilages appears to have been the first event in the process. It is to be observed, however, that it must not be concluded from every case of laryngeal ulceration, in which the cartilages are found ossified and carious, that the ulcerative process was occasioned by previous disease in the ossified cartilages. This may happen to be the correct relation or order of events; but it may be merely a coincidence, or, in some circumstances, a consequence of disease of the soft parts. Ulceration of the larynx may be the consequence of acute inflammation, which has terminated without suffocation, or of chronic inflammation, occasioned by any of the usual exciting causes. In such circumstances, purulent matter is found in the ventricles of the larynx, the mucous membrane is detached by irregular ulceration, and some thickening, ascribed to strumous action, is found in the contiguous and subjacent tissues. * Ulceration of the tracheal membrane is too frequently the result of chronic inflammation, which is here remarked to occur in strumous habits, or at least in persons who are very liable to have catarrhal and chronic affections of the mucous membranes in general; and as it is often combined with ulceration of the œsophagus, it has been presumed to have commenced in this tube, and thence extended to the trachea. †

The termination of these cases is generally unfavourable; yet an ulcer of the larynx or trachea appears to be as much under the influence of medical management, as in any other part of the body. That there is nothing in the nature of the tissues of which the larynx and trachea are composed, which should prevent the process of restoration, we have every reason to conclude, not only from the facility with which wounds of these tubes, whether accidental or operative, are healed, but also from the instance of tracheal consumption mentioned by Borelli, and from the remarkable case related by Dr Lettsom, in which the foil of a button, which had dropped into the windpipe, gave rise to all the symptoms of consumption, which disappeared as soon as the foreign body was spit up. ‡ It is impossible to conceive that such a substance should continue for four months in the windpipe, without producing ulceration, and equally imposssible to suppose that recovery could

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* Baillie, 91. † Baillie, 98.

‡ Memoirs of the Life and Writings of the late John Coakley Lettsom, M. D. &c., with a Selection from his Correspondence. By Thomas Joseph Pettigrew. Vol. iii. p. 82. Art. XIII.

take place unless the ulceration had been cicatrized; and we are forced to conclude, that there must be in the constitution of the individual, or in the treatment, some circumstances which exercise a great influence in preventing or promoting this favourable result. In a case related by Heller, in Thomann's Annals, recovery took place under the use of sulphuret of antimony and hyoscyamus, with a restrained diet, composed chiefly of Iceland moss, and afterwards with inunction over the throat of an ointment containing calomel and opium. * Many cases however show, that the great local action or irritation is a principal agent in preventing the sanative process; for when this is removed by any means whatever, the symptoms have subsided, and health has been restored. Such appears to have been the case in the instance recorded by Morgagni,—at least if it be admitted that the symptoms were occasioned by an ulcer of the tracheal membrane. †

Ulceration of the trachea occasions death either by its effects on the general health, which is perhaps the most common mode,—or by the ulcerative process establishing a communication between its cavity and that of the œsophagus,—and thus producing suffocation. Ulceration of the larynx, besides these two modes of causing death, may do so in a third, which we rather think is its most frequent mode of termination. This is by its concomitant inflammation affecting the investing membrane of the arytenoid cartilages, or the substance of these bodies themselves. The result is remarkable. As these cartilages and their investing membrane form the narrow slit or chink termed the *glottis*, and as a very slight degree of swelling obliterates the opening, suffocation may suddenly take place, and be followed by death. This is indeed to be regarded as a secondary form of inflammation of the larynx, so suddenly and generally fatal. In all the examples of laryngeal consumption which we have seen terminate fatally, and which we have had opportunity of examining after death, we have found the arytenoid membrane swelled and thickened by effusion of fluid beneath it, and the aperture of the glottis much or completely obliterated. In some of these cases it is worthy of remark, that points of the laryngeal membrane, which had been in a state of ulceration, had undergone a process of repair, and had been cicatrized. In other parts, a sort of fungous granulating substance had risen from the place occupied by ulcers. In the cases recorded in the Medico-Clinical Annals of Wirceburg, to which we have already alluded, two terminated fatally; the third recovered. In those which terminated fatally, the mucous membrane covering the epiglottis, and the arytenoid cartilages of one side, were removed by ulceration, which had extended also over the velum, uvula, and part of the pharynx. In one, the membrane between the epiglottis and arytenoid cartilage was destroyed by ulceration. In the other it was ulcerated; the margins of the glottis were ulcerated and warty, and the upper thyro-arytenoid ligament of the left side was completely destroyed. In a case recorded in the Memoirs of the London Medical Society, of four months duration, the epiglottis and upper part of the trachea was ulcerated, and the hyoid bone was necrosed, exfoliating, and carious. *

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* Annales Instituti Medico-Clinici, &c. Vol. i. p. 171.

† De Sedibus et Causis Epist. xxii. 27, 31.

III. It has been the subject of much controversy among medical persons, whether the pleura can be inflamed without the same process extending to the lung; and whether pleurisy can exist without peripneumony. The pathological collections of Haller and Morgagni have been supposed, by many authors, to prove the contrary, and to establish the inference, † that, though a portion of lung may be inflamed without affecting the pleura, it is impossible for the pleura ever to be affected without a similar state in the contiguous portion of lung. To the nosologist who feels himself obliged to create a disease from symptoms, this distinction is perhaps of some importance; but to the pathological physician, who fixes his observation on the changes incident to the organic tissues, and connects them with the exterior signs to which they give rise, it is of no other use than merely as a fact of which he must be aware, and the value of which he will appreciate in studying the successive phenomena of any morbid process. Since, however, it may be considered as established, that the pleura is often inflamed without affecting the pulmonic substance, the name *pleurisy* must be restricted, according to the distinctions of modern pathology, to inflammation of the pleura only. This disease may exist in every variety from the most acute, which runs its course in five or six days, to the most chronic, which continues for weeks and months—in some instances more than a year. In the first case, the rapidity of its progress, and the violence of its effects, rarely allow it to give rise to phthisical symptoms; but when the disease, either by becoming chronic, or by being originally so, has lasted for weeks in the same or similar tenor, the symptoms, which had at first been those of symptomatic fever, are gradually converted into hectic, with its usual concomitants of wasting and loss of strength. It is therefore to the chronic forms of pleurisy that our present inquiries pertain.

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* Memoirs of London Medical Society, Vol. iv.

† Haller, Opusc. Path. xiii. xiv. Morgagni, Ep. xxx. Pringle, 142.

The serous membrane of the lungs, when inflamed chronically, presents nearly the same anatomical characters as it does in the acute state. The membrane becomes of a red punctuated or dotted appearance, with minute points of a deeper red than in the acute disease, but of the same irregular figure, the spaces between of natural colour; but the membrane is also traversed by numerous vessels red or dark coloured, but quite distinct, and not colourless as in the natural state. Many pathological observers, and among others Dr Baillie, have described the pleura as thickened when in a state of inflammation, and have considered this change so usual, as to be deemed a necessary consequence of the inflammatory process. This effect has been denied by M. Laennec, who informs us that he never could perceive the membrane distinctly thickened; and asserts, that the phenomenon described by these authors as *thickening*, is either an extensive congeries of miliary tubercles on its free or adherent surface,—or a cartilaginous incrustation on the parts covered by the membrane,—or, lastly, false membranes more or less dense, adhering closely to its free surface.

The red-punctuated appearance may be regarded as the first or incipient part of the inflammatory action occurring in the pleura. It extends through its thickness, and continues as long as the disease lasts. But it never continues long without being attended with the formation and effusion of new matter from the free surface of the membrane. The first and most usual of these is the gelatinous or rather albuminous substance, denominated by J. Hunter coagulating lymph. When effused, this substance is always semifluid, viscid, of a yellowish colour, and not unlike imperfectly coagulated animal jelly. In a short time it is penetrated with vessels, shrinks or diminishes in size, becomes firmer, and acquires the vital property of exhalation. It then constitutes what has been named false membrane; and if, as frequently takes place, it is united to the corresponding part of the opposite pleura, it is termed *membrane of adhesion*, or simply *adhesion* (concretio, concrementum.)

The second species of matter formed by the inflamed pleura, is an opaque fluid of grey, or cream-like aspect, but not uniform in consistence. If allowed to stand, it separates into a thin watery fluid, which resembles green whey or milk much diluted with water, more accurately than any other thing; and a thinner matter which remains at the bottom of the vessel, and which, on examination, is found to consist of different kinds of substance, the proportions of which vary according to the duration and degree of disease. The first of the constituents of this thick substance appears to be merely the same kind of fluid as that which was decanted from it, and, when properly separated, appears to differ in nothing unless in being mixed with the other matters. The second ingredient which we shall notice, is a thicker semifluid substance of minute globules or grains, which appear to float, or be mechanically suspended in the more fluid portion. This appears to be a modification of purulent matter, and may be regarded as the purulent fluid properly secreted by the inflamed pleura. The third ingredient of this fluid consists of shreds or patches of curdy or albuminous matter, which is evidently of the same nature as that which forms the exudation of the membranous adhesions.

In acute pleurisy, besides the red appearance of the membrane, lymph is effused, and more rarely the sero-purulent fluid which we have described. But in the chronic disease, whether it has existed so from the beginning, or has supervened on an attack of acute inflammation, this fluid continues to be effused for a considerable time, and its presence constitutes the distinguishing pathological character of the disease. It is to be regarded as the immediate effect of the process going on in the pleura, and it will continue to be effused as long as this process continues. While the effusion of these morbid fluids from the pleura goes on, the increasing quantity breaks down whatever membranous adhesions are not sufficiently strong, and separates the lung from the inner surface of the chest, with which it had been previously in immediate contact. It thus compresses the organ more and more daily, until, when it has filled almost the whole cavity of the pleura, the lung is reduced to so small a size, that, on examination, it would seem to have been completely destroyed. After death, however, when the matter is removed, it will be found forced up towards the mediastinum and spine, and reduced to a very small compass indeed. In consequence of this compressed or squeezed condition, the lung becomes much smaller, its vessels are emptied, or at least the blood is prevented from filling them and moving freely through them; its bronchial tubes are crushed together, so as to prevent the admission of air beyond the first divisions; and the whole organ is rendered unfit for the purposes of respiration. If a lung which has been long subjected to this pressure be examined, it will be found not to crepitate, or at least indistinctly, to float imperfectly, and to be incapable of inflation by its bronchia, or complete injection by its vessels. This is the condition of lung so often mentioned by Broussais in his Cases, under the name of *atrophied lung*,—19, 20, 24, 25, 27, 28, 30.

In some instances of this author, the lung is also said to be reddened and hardened, or hepatised. It is easy to see that it must be impossible for a substance so light and elastic as lung to be forced into the small space mentioned in such cases, without becoming much denser, heavier, and more solid. But we conceive this state ought to be entirely distinguished from that which we have already described, when enumerating the pathological changes incident to the organ in chronic catarrh. That was shown to consist in an increased quantity of matter in the same space, without diminution of volume; in other words, by the deposition of new substance in the interstices of the old. The change of which we speak at present, consists in approximating more closely the particles of the old matter, or in forcing it to occupy a smaller space.

In the course of this process, various events may take place, which, as they are more or less intimately connected with chronic pleurisy, may be noticed shortly.

1st, Suppurative destruction may take place in one point of the pulmonary pleura, and the corresponding part of the lung, by means of which one or more bronchial tubes are laid open, and a considerable quantity of purulent or sero-purulent fluid is discharged in certain positions by coughing. This fluid always comes up in considerable quantity, sometimes in a continuous stream, as if discharged by vomiting; but the sensations of the patient, and the distinct coughing, show that it comes through the windpipe. The time and the quantity of this discharge, will depend much on the direction of the communication between the bronchial tubes and pleura, and on the manner in which it is made. In some instances which have fallen under our observation, the capricious irregularity of its appearance was explained after death by the appearance of a sort of valvular apparatus, consisting of coagulable lymph, placed very near the pleural end of the fistula. We have reason to believe, that this is an event not uncommon in the course of chronic pleurisy; for we have seen two or three examples of it in the course of some years. *

2d, Suppurative destruction may take place in one or more points of the costal pleura, and a quantity of the sero-purulent fluid, more or less considerable, is expelled through openings between the ribs. Previous to this, in general, the effect of the effused fluid on the side becomes manifest. It becomes enlarged; the intercostal spaces become broader, and rise to a level with the ribs, or even above them. At the same time, the integuments become œdematous, and the cutaneous veins are much enlarged. This is a less frequent occurrence than the former; but several examples have been recorded by physicians, at no remote period. Dr Hunter's collection contained a preparation, in which matter had been discharged through several openings in the intercostal spaces; and we may refer for examples to the Miscellan. Curios. sive Ephemer. Decur. III. An. V. Obs. 49. Memoirs of Med. Society, vol. III. p. 127.

3d, We have already mentioned the effect which the effused fluid produces on the size of the lung, and shown that it forcibly compresses this organ toward the spine and mediastinum. We have now to remark, that when chronic pleurisy occurs in the left side of the chest, as the quantity of sero-purulent fluid increases, it not only compresses the lung, but frequently displaces the heart so much, that the pulsations of this organ are not felt at its usual situation on the left side, but first close to the sternum, and afterwards on the right side entirely. We have met with two cases in which this change of situation had taken place, and in both it was occasioned by the great quantity of purulent fluid filling the sac of the pleura. When it occurs, it is liable to be mistaken for serious disease of the heart; but examination shows, that the change of pulsation is caused merely by forcible displacement, without change of structure of the organ. *

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* See Morgagni, Epist. XX. 6, where a good case with dissection is given.

The knowledge of circumstances of this kind is not only necessary in showing the true nature of this malady, but is also not without its use in inquiring into the organic process by which the disease and its effects are removed. This inquiry resolves itself into three points; 1*st*, Whether the chronic inflammation of the pleura has been, or may be checked, under a course of management; 2*d*, Whether the fluids which it has formed can be removed from the pleura by the powers inherent in the living tissues; 3*d*, Whether the lung which has been compressed can be restored to its original state, and be again fit for the purposes of respiration.

That the process of chronic inflammation may he checked or suspended in the pleura, appears to be quite as possible as that it should be so in other tissues and organs of the human body. But there are circumstances here which operate in a manner totally different from what is observed in other situations. 1*st*, When fluid has been once effused, it appears to operate like a foreign substance, and aggravate, in some instances, the original disease. If it be considerable, it breaks down the layers of lymph which otherwise connect the pulmonary to the costal pleura, and prevent the further extension of the disease; and by compressing the lung, it renders the chance of removing the disease much less likely. 2*d*, The removal of the effused fluid by absorption, is a very precarious and uncertain event; and though it has been known to occur, it has not been ascertained what are the circumstances which favour this termination. Some recent observations would appear to show, that the same agents which check local inflammation, are the most likely to be beneficial. Notwithstanding these obstacles it appears, that chronic inflammation of this membrane does in a few instances cease; its effects disappear, and the individual recovers a considerable degree of health, in some instances is completely restored. The cases hitherto recorded show, that one of three events is requisite to this termination. Either, on the cessation of the inflammatory action, the fluid effused is gradually absorbed, while the lung expands, and the lymph with which its membrane is covered, forms points of adhesion with the pleura; or a communication between the pleura and bronchial tubes takes place, by which the effused fluid is discharged by expectoration; or an opening takes place in the intercostal spaces, and allows it to escape in that manner. If the disease terminates in the first of these modes, a very peculiar change generally takes place in the chest, which has been best described by M. Laennec under the name of Contracted Chest. As we had occasion to speak of this formerly, when examining the pathological researches of this author, we satisfy ourselves with this notice of it, and referring our readers to that article,* and the work of M. Laennec. We have only to remark on this point, that one of Dr Beddoes's consumptive cases is evidently an instance of chronic pleurisy, in which this sanative process had partially taken place,† and the explanation of which is certainly due to the researches of M. Laennec.‡

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* Since the above was written, another instance of the facility with which the effect thus mentioned may be ascribed to disease of the heart, is placed before us. In the volume of the Transactions of the Medico-Chirurgical Society of this place, just published, the concluding case of Dr Abercrombie's valuable paper on the Pathology of the Heart, is a very distinct example of chronic pleurisy, though given as displacement of the heart. This organ was undoubtedly displaced; but the change of position was a mere effect of the sero-purulent effusion, which again was the effect of the original disease,—inflammation of the pleura. *Trans. Medico-Ch.* pp. 66 and 67. See also an interesting case in Memoirs of Medical Society, Vol. v. p. 215.

When chronic pleurisy terminates by a fistulous communication between the pleura and bronchial membrane, a degree of pneuma-thorax generally takes place, unless the lung expands with the same rapidity with which the sero-purulent fluid is discharged from the cavity. In most cases which eventually recover, membranous adhesions seem to prevent this from being so considerable as to compress the lung; and, indeed, the equality of temperature in the air which escapes, and in that which is contained in the pulmonary vesicles, generally operates as a resisting power. If the pneuma-thorax be considerable, it may retard, or ultimately prevent recovery. In some instances, the expanding power of the lung itself seems to force out the sero-purulent fluid, without admitting any air to the cavity of the chest. This is a form of consumptive disease by no means uncommon.

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* Med. and Surg. Journal, Vol. XVIII. p. 462.

† Observations on the Nature and Cure of Calculus, Sea-Scurvy, Consumption, &c. By Thomas Beddoes, M.D. 1793. p. 146; and a Letter to Erasmus Darwin, M.D. on a New Method of Treating Pulmonary Consumption, &c. By Thomas Beddoes, M.D. pp. 23 and 24.

‡ Laennec, by Dr Forbes, p. 159. Sect. IV.

We may here notice another variety of this mode of termination. Not only may a fistulous opening take place between the bronchial membrane and pleura, so as to admit air into the cavity of the latter membrane, but the cellular tissue of some point of the walls of the chest may be laid open by ulceration, and, air escaping into it, may form an emphysematous swelling of more or less of the surface, with or without discharge of purulent matter. This termination is well illustrated by a case of pleurisy terminating in emphysema and consumption, described by Treyer in the 1st volume of Thomann's Annals, * and by a remarkable case, in which there was a fistulous communication, openings through the parietes of the chest, pneuma-thorax, and emphysema, described by Dr Duncan in the 1st volume of the Transactions of the Medico-Chirurgical Society of Edinburgh. †

If it were possible to form a decided opinion from reports of cases already recorded, we should say that termination of chronic pleurisy, by spontaneous opening through the intercostal spaces, was by far the most common, and perhaps the most favourable. Its event, however, is precarious; and the great variety in this respect will perhaps prove to depend on the condition of the lung, and the degree in which the constitution is affected. When the lung is sound, and without tubercular or other destruction, if none of the ribs be carious, a favourable result is more probable than in the contrary circumstances. It is easy to estimate the degree of danger depending on other causes; and we have only to refer to the following examples, in which the disease was almost invariably regarded as consumption which had been cured. Miscellan. Curios. sive Ephemerid. Decur. III. An. V. Obs. 49, said to be consumption cured by nature. Warner's Cases in Trans. Roy. Soc. vol. XLVII. p. 407. XLVIII. p. 270, successful. Vol. LI. p. 194, fatal. Memoirs of Med. Society, vol. V. p. 215, fistulous opening and incision, recovered. Vol. V. p. 309, incision, fatal.

We conclude this part of our subject with two observations; 1st, The history of chronic pleurisy, and of its pathological characters, shows that physicians have overlooked its importance and true nature, and have more generally attended to its effects than to its intimate and proper character. In the writings of authors, both practical and pathological, it is designated only by one of its effects,—the effusion of sero-purulent or sanguineo-purulent fluid (empyema, some cases of hydrothorax); and we rather think that physicians have too often allowed the primary

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* P. 176. † P. 455.

pathological cause, inflammation of the pleura, to be completely forgotten in the result or consequence of its action. Empyema and hydrothorax are indeed not so much diseases, as the effects of one and the same morbid cause; and it is equally inconsistent with rational pathology, or the sound observation on which it depends, to create as many distinct maladies as these variations in the action of that cause in which the disease truly consists. 2. There is reason to conclude, that several of the cases recorded as pulmonary abscesses expectorated and cured, were examples of chronic pleurisy terminated by the formation of pulmonary fistula. We shall see that Laennec denies the ordinary occurrence of pulmonary abscess; and if it be so rare as he contends it is, or even as we ourselves allow it to be, the cases recorded admit of satisfactory explanation, in no other way than by referring them to this cause. Such may have been the case related by Dr Wright in the Transactions of the Royal Society, generally quoted as an instance of pulmonary abscess; and we believe that the reading and experience of many of our professional friends could easily furnish them with similar examples.

IV. The formation of a distinct abscess of the lungs as a consequence of inflammation, was at one time generally admitted among pathologists. Laennec, however, who describes suppuration of the lungs under his third degree of pulmonary induration, maintains that it is exceedingly rare, and gives it as the result of his observation, that small abscesses are found in the pulmonic tissue not above four or five times, and an extensive one not above once, in many hundred cases. All the reported cases of pulmonary abscess, or suppuration of lung, as a consequence of inflammation, he regards as excavations or *vomicæ* formed by the softening of extensive tubercular masses. It is possible that suppuration, as a consequence of inflammation of the lungs, may be rare, for two reasons; 1*st*, Because the disease may prove fatal by suffocation, before it has attained the complete suppurative stage; 2*d*, Because, under the influence of remedies, it may be so much modified as to prevent the formation of purulent matter in a distinct sac or cavity. But we do not regard it as so rare as M. Laennec appears to represent it. Instances are recorded by Morgagni, in which a considerable portion of the pulmonic tissue was converted into a purulent abscess, with the contiguous structure apparently healthy,—or indurated as a consequence of previous inflammation.* Dr Baillie expresses himself with some uncertainty; for his language may be interpreted so as to apply either to tubercular vomicæ, or to pulmonary abscesses; though it is evident, and more especially from what he says in his engravings, that he believed in its ordinary occurrence. Is the preparation which he has delineated in his Vth Engraving, p. 37, to be regarded as an instance of it? Is the case recorded by M. Foubert, in the Memoirs of the Academy of Surgery,* or that by Dr Wright in the Transactions of the Royal Society,† or that related by Heller in the Annals of Thomann,‡ to be regarded as examples of pulmonary abscess? That by Dr Wright might have been an instance of chronic pleurisy with pulmonary fistula; and there is scarcely a case which might not be explained without supposing a true pulmonary abscess. In what light are we to view the imposthumes described by Dr Bisset, in his correspondence to Dr Lettsom?‖ There is a defect of evidence on this subject; and we require several good cases, with the appearances after death, in order to ascertain the frequency or the general occurrence of abscess as a consequence of pneumonic inflammation.

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* Epist. LXI. 2. XXVIII. 12.

Though we have considered induration or consolidation of the lungs as a consequence of long continued bronchial inflammation, we must here observe, that it may occur primarily as an effect of chronic peripneumony. Without dwelling longer on the pathological properties of this change, we have merely to remark, that various facts prove that this chronic inflammation gives rise to phthisical symptoms so well marked, that it is impossible to distinguish them by the usual means from those occasioned by tubercular destruction. The most satisfactory instance is that described by Dr G. Pearson, to which we have already alluded.

The pulmonic tissue appears to be subject to a particular form of inflammation terminating in formation of matter, but occurring in many minute points. We have met with two or three examples only, in which, without expecting any morbid appearance, we found the pleura sound, the lungs interspersed at considerable distances with numerous minute abscesses, but the intermediate tissue quite healthy. As it occurred that these were softened tubercles, the whole organ was carefully examined, yet without finding any thing but minute spherical abscesses of various sizes, and with the surrounding texture natural. The peculiarity, therefore, of this species of suppuration, is its not being preceded, so far as could be ascertained, by tubercles, the pulmonic tissue neither inflamed nor indurated, and the simultaneous formation of many purulent points.

Is this to be regarded as a pustular inflammation of the lung?

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* Tome i. of 4to, iii. of 12mo.

† Vol. xxiii. p. 1378.

‡ Vol. i. pp. 215 and 224.

‖ Memoirs of Life and Writings of Dr Lettsom, Vol. iii. p. 315.

Will it explain the appearance of vomicæ in the lungs of persons who were never suspected to have tubercles? Is it a disease peculiar to strumous habits? This appears to be the form of organic change which Dr Baillie mentions in the following terms.

"I have sometimes seen a number of small abscesses interspersed through the lungs, each of which was not larger than a pea. The pus there is rather thicker than what arises from common inflammation, and resembles scrofulous pus. It is probable that these abscesses have been produced by a number of small scattered tubercles taking on the process of suppuration. The lungs immediately surrounding these abscesses are often of a perfectly healthy structure, none of the cells being closed up by adhesions." †

Notwithstanding the opinion of this pathologist, that these were the result of scattered tubercles, it may be urged, that had this been the case, some unsuppurated tubercles might have been expected to be found, or the surrounding tissue might have been supposed to be indurated after such a slow and tedious process as the tubercular softening. Nothing of this kind, however, was remarked in the few cases which we have seen; nor does the description of Dr B. himself warrant any such conclusion. We know nothing of the possibility of these abscesses being either absorbed or discharged by expectoration, and the pulmonic tissue being restored. The state of the lungs would lead us to imagine, that recovery might be very probable. The whole subject requires more attentive and accurate investigation.

V. The pathological characters of pulmonary tubercles have so often on late occasions been brought under notice, that detailed inquiry at present is quite superfluous. While, therefore, we refer to our articles on the works of Mr Lloyd, M. Laennec, and Dr Baron, our observations in this place may be confined to a very narrow compass. The points of inquiry most important to be ascertained are, 1. Whether the presence or formation of tubercles gives rise to any changes in the functions of the living body, which may serve as pathognomonic signs of their existence; 2. Whether after formation they may continue latent or quiescent, that is, without seeming to undergo any changes tending to destroy the lung. 3. Whether medicine possesses any means of controlling this action; and, 4th, Whether the process of tubercular disorganization can be arrested or subdued.

It is most unfortunate, that the very matter on which all the other subjects depend, is involved in the utmost ambiguity; for it is well known, that in morbid dissections we daily meet with tubercles, which no symptom had led to be suspected, and even on the strictest interrogation from friends, or those who were in the habit of seeing the patient daily, it is impossible to obtain any certain proof that he was out of health. On the other hand, we often meet with cases in which phthisical symptoms are developed suddenly, and without previous indisposition, and continue till the fatal event, after which both tubercles and vomicæ are found in the lungs. These facts, which are well authenticated, and various examples of which are scattered through the works of pathologists, lead to one of two conclusions; 1. That tubercles may exist in the human lung without either occasioning local inconvenience, or affecting the general health; or, 2. That their formation is very rapid, and accompanied in many instances with the usual phenomena of inflammation. It is for this reason that many pathologists believe that tubercles may continue long without producing local uneasiness, or, in other words, in a latent or inactive state; for it is difficult to imagine, that bodies like tubercles could be formed in the short space which elapses between the first appearance of illness, and the complete formation of phthisical symptoms.

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† Morbid Anatomy, p. 72.

The ambiguity and obscurity indeed in which this subject is involved, renders it quite impossible to fix on a single point, from which we might proceed with confidence and certainty in the investigation; and we have therefore no means of knowing whether tubercles are already formed in any given case, or whether, when formed, they may be absorbed. We are unwilling to encourage the opinion, that the veins or lymphatics are unable to remove by absorption, such bodies as dissections show tubercles to be; but the records of medicine afford no unequivocal proof that a tubercle or tubercles ever were absorbed. In those cases in which the symptoms led to the belief that tubercles were forming, and had disappeared, it may be said, that the same symptoms might be occasioned by several varieties of disordered health, and, at all events, by chronic peripneumony or pleurisy. These considerations, which are founded, we may say, on strict matter of fact, show the exact bounds to which medical knowledge extends on the three first questions which we stated above; and the physician is compelled, in the present state of information, to abandon all speculation or reasoning on the tractability of the disease, or the powers of art in its early period, before it has assumed an active and unequivocal aspect.

It is therefore to that stage of their progress, which the symptoms have led physicians to denominate *active*, that the attention of the pathological inquirer must be directed with a view to ascertain the tendency of these bodies. And we find there is almost as much difficulty in determining the precise point at which quiescence ceases and action begins, as there is in ascertaining the presence of tubercles in any given case. It was long supposed that the appearance of purulent matter in the expectoration was the proof of tubercular softening, or suppuration. We have already seen, however, that purulent matter may be discharged without coming from tubercular vomicæ; and we have now to mention, that more careful observation, united to the aid afforded by morbid anatomy, has shown, 1st, That the process of tubercular softening is accompanied with considerable variety of expectoration; 2d, That it may be far advanced without being suspected by the symptoms; and, 3d, That there are few cases indeed in which it is not complicated with chronic bronchial inflammation, and in which, consequently, it is not attended with copious mucous or muco-purulent discharge, totally independent of that which comes from the tubercular matter.

According to the observations of Dr G. Pearson, whose authority we have already quoted, it appears, that tubercular deposition in the lungs may be attended with a variety of chronic inflammation of the mucous membrane, more or less local, in which the opaque ropy matter, which forms the third sort of his expectorated fluids, is secreted very copiously. This symptomatic action, which is popularly known under the name of *winter-cough*, may return and subside every winter for years, without coming to a fatal termination; and though tubercles are the remote cause of the expectoration, they are not its immediate source. This is to be understood of tubercular action in all ages, but chiefly after the period of youth. * The pulmonary consumption of young persons, depending on the presence of tubercles, is also attended with similar expectoration proceeding from the same cause,—a secondary local inflammation occasioned by the obstruction which the tubercles cause in the pulmonary circulation. This fluid has been often mistaken for the matter of softened tubercles; but dissection has shown this to be erroneous; for in the lungs of such subjects, though tubercles were numerous, and sometimes beginning to undergo the usual changes, none were broken, nor could any communication be traced between them and the bronchial membrane.

" The substance of which I am now speaking, " says Dr Pearson (puriform matter, the 4th sort), " is most frequently excreted in the latter stages of pulmonary phthisis for many weeks successively. It is taken for granted, that this matter is from a breach of surface or ulceration; but on examination after death, such a state was not found in many instances, under my observation, although the lungs were as usual full of tubercles and vomicæ. " †

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* Transactions of the Royal Society, 1809, 1810. † Ibid. 1809, p. 320.

These facts, which are well authenticated, prove that, in many subjects at least, tubercular deposition, when it has attained a certain point in its progress, occasions a true bronchial inflammation, which may continue till the fatal event; and that the matter expectorated does not proceed from the tubercles, but from the inflamed bronchial membrane.

That enlargement, and the ulterior changes of tubercular destruction, may be much advanced without producing unequivocal symptoms, is so well established by the observations of Laennec, that it is unnecessary to dwell longer on it.

That tubercular disease of the lungs is complicated, generally in the early stage with chronic catarrh, and always in its latter stage with this and inflammation of the contiguous pulmonic tissue,—follows directly from what has been now stated; and these statements have been confirmed by the observations of Laennec. It is important, therefore, for the practical physician to remark, that every case of tubercular phthisis consists of at least two, sometimes of three simultaneous morbid processes; 1st, The proper tubercular action, which may either be in the stage of growth or increase, or in that of softening or destruction; 2d, Of a degree of chronic inflammation of the pulmonary mucous membrane; and, 3d, Of local inflammation of the pulmonic tissue, generally chronic, and tending to produce consolidation of the lung. Of these, the two latter only are in some degree under the control of medicine; for it is distinctly proved by the observations of Bayle and Laennec, that the process of tubercular softening has never yet been amenable to art, and that, when once commenced, it continues until the whole is discharged. The researches of the last of these writers, however, have shown, contrary to what had been generally supposed, that at this period recovery may take place in two modes,—by the cavity becoming either invested by a new membrane, or obliterated by a cicatrix more or less complete, consisting of cellular, fibrous, or cartilaginous substance.* We have already noticed this mode of termination in our account of M. Laennec's work; and it is unnecessary to dwell longer on it here, than merely to say,—that the mode in which it is accomplished, is very different from that which has been hitherto supposed to indicate the cure of consumption, and which we have shown belongs properly to recoveries from chronic catarrh,—and does not afford that strong evidence of the sanability of genuine consumption which some pathologists are resolved to draw from it. It is obvious that the chance of a patient recovering in this mode, will depend, *1st,* on the time occupied in the discharge of the tubercular matter, and, *2dly*, on the number of tubercles in the lung ready to undergo the same change. If the health and strength of the individual be much reduced, during a long and tedious process of tubercular suppuration, or if many tubercles have to undergo the same changes, it is physiologically impossible for recovery to take place. When the state of the lungs in subjects affected with tubercular deposition is remembered, it will not require much argument to prove, that the proportion of recoveries even must be small indeed; for the majority of instances consists of cases in which not one or two, but many tubercles are simultaneously undergoing the same changes. Even the case of wound and cicatrix of the lungs, which has been supposed to encourage the hopes of curing consumption, does not here admit of application; for, while the inflammation and suppuration succeeding a sword-thrust, or the introduction of a pistol-bullet, are limited with more or less accuracy to one region of a single lung, or even of a single lobe,—cases of genuine pulmonary consumption generally depend on the presence of many tubercles, or tubercular vomicæ in the same lung, or even in both; and it is not the effect of one, but of the whole, that contributes to the fatal termination.

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* Laennec by Forbes, p. 28.

A single observation of importance in a practical view must not be omitted. To what extent should the efforts of the physician be carried, when he has no means of acting directly on the morbid process? Should he attempt to moderate or remove the local inflammation or congestion which depends on it? Or, is he to leave the whole to the constitutional powers of the individual? We are inclined to think that these limitations form the basis of the treatment of consumption; that the chief object of the physician is to moderate, as much as may be, the bronchial inflammation, or pneumonic congestion; but that, in doing so, all means should be avoided which are likely to impair the general strength. Two evils, indeed, are to be avoided, neither to carry the antiphlogistic means of subduing the secondary inflammation so far as to impair the powers of the system; nor to administer those stimulating and irritating remedies which, whatever effect they have on the sensible strength of the individual, or even on the tubercular action, always aggravate the secondary inflammation.

The view which we have here taken furnishes several practical conclusions, which we trust it will not be unseasonable to notice briefly at present. It shows, in the first place, that the semeiography given by practical and nosological authors is imperfect, and affords the practitioner no certain means of distinguishing the precise nature of that process, which is going on in the lungs of his patient. It demonstrates, among other things, the inutility of the much laboured diagnostic of purulent expectoration, and that the physician can no longer trust to this as a means of ascertaining the probable state of his patient's lungs. An immediate and natural result of this is, that he may either be destitute of any certain means of applying the remedies suited to the particular variety of morbid action, or by supposing it beyond the reach of art, when it is still under control, may indulge in the hopeless indifference of inactivity. It is obvious, therefore, that it is requisite to look for diagnostic means more positive and particular in the information which they afford; and in which confidence may be reposed, both in ascertaining the effects of remedies, and the kind of curative means which are most appropriate. In this difficulty, it is unfortunate that not even the stethoscope can give us satisfactory information of the presence of tubercles previous to excavation; but, by negative evidence of their absence, and positive evidence, in many instances, of other morbid conditions of the organ, information much more certain can be obtained than by any other mode. When all the usual signs of consumption exist, and concur to perplex the physician, and prevent him from ascertaining whether these symptoms are occasioned by chronic catarrh, or tubercular softening, or from distinguishing between chronic pleurisy and the early stage of tubercles, he may rely on the evidence of the stethoscope with the greatest confidence. In other instances, as in fistulous opening into the pleura, and actual tubercular excavations, the necessity of discarding our ancient semeiography, and employing the less ambiguous aid of the stethoscope, is still more obvious. It is unnecessary to prolong these observations; but we trust they have not been completely useless, if they shall be the means of directing the attention of even a few of our readers to the importance of distinguishing, in practice, those pulmonary disorders which, though indicated by nearly similar symptoms, arise from different morbid states, and require varieties of treatment.

IV.

A Treatise on Nervous Diseases. By JOHN COOKE, M. D. &c. Vol. II. Part II. *History and Method of Cure of Epilepsy.* Longman, &c. London, 1823. pp. 235.

NOT long ago we ventured to recommend to our readers the former parts of Dr Cooke's work on Nervous Diseases, as constituting one of the best examples of successful compilation which have come under our notice. In the present state of the medical sciences, which, within these few years, have been enriched on all hands with an unprecedented mass of new and interesting information, it is no small obstacle to the zeal of the most diligent student, that the facts which have been discovered remain dispersed throughout a most irksome multiplicity of sources, and that he has also to struggle against the conflicting testimony of authors as to their truth and import. The labour of the compiler, therefore, has of late become by no means an easy or an unimportant one; and he appears most justly entitled to the reward now so generally bestowed upon success in this kind of composition. Judging from the specimens which have been already laid before the public eye, we have felt no hesitation in ranking Dr Cooke among the very first authors of this description. His patient and accurate research, his rare and happy talent in choosing and estimating his materials, his dignified manner of handling controversial topics, and his perspicuous and classical style, peculiarly fit him for the responsible duty of collecting and displaying in attractive colours, for the sake of those numberless persons who have neither time nor opportunity to collect for themselves, whatever modern science presents that is directly applicable to the practice of our profession.

The present work of Dr Cooke will not occupy us long. It is the continuation of his projected Treatise on Nervous Diseases. We are sure the author will not claim for it the almost unqualified approbation, which we have bestowed with pleasure on his former undertakings. For he does not profess to advance in it any new facts or any novel points of doctrine; and, viewing it as a simple compilation, it cannot be pronounced, in reference either to its scientific character or its practical utility, as at all comparable with his treatises on Apoplexy and Palsy, or even as sensibly superior to other modern works on Epilepsy. This, indeed, is unquestionably to be ascribed not to the author's incapability of travelling through all the intricacies of his subject, and of gathering with his accustomed skill the choicest fruits which he meets with in his progress,—but rather to the sterility of the regions he had to traverse, and the doubtful quality of its productions. But, although these considerations will probably wipe off any stain, which, in our estimation, the present work might tend to fasten on the kind of merit usually conceded to him as an author; it will not excuse him for having thrown away his peculiar talent on a subject so little congenial with it. A systematic practical treatise can be written advantageously on those subjects only, which have arrived at a reasonable degree of perfection and stability. Unless the chief facts and doctrines regarding a disease are tolerably stable, and its treatment either reasonably successful, or at least tolerably uniform, it is evident that the labours of the compiler must fail in their principal objects, namely, in settling the opinions and assimilating the practice of the profession, and in directing those who are accustomed to seek for direction from acknowledged authorities.

On these grounds it may be safely affirmed, that the medical world is not yet prepared to receive a systematic compilation on the subject of Epilepsy. The compiler must encounter in his progress too many gaps, which he cannot fill up from any legitimate source. In the present state of our knowledge concerning the pathology of this disease and the application of Therapeutics to it, we presume no treatise will be acceptable or useful, which does not consist, in part at least, of a collection of new facts, or of a statement of precise tests by which we may try the value of those hitherto collected. We have already said that Dr Cooke does not profess to state any thing new on the subject; and we may appeal to any one who has waded through his account of the causes of Epilepsy, or through the farrago of remedies which fill seven-twelfths of this work, whether he has advanced us a single step in the estimation of formerly-received facts.

Such as it is, however, the present work exhibits many proofs of our author's masterly hand, of his correct taste, and of his nervous and faithful description. We would refer, in particular, to his account of the symptomatology, which is admirable. Take, for example, the following beautiful, though in some respects overstrained description of the Epileptic idiot, extracted from his favourite ancient, Aretæus.

" If the disease be of long duration, patients suffer from it, even in the intervals of paroxysms. They become torpid, languid, and dejected; they avoid the sight and society of men; time does not afford any mitigation of their sufferings; they are often oppressed with watchfulness, and when they do sleep, they are terrified with horrible dreams; they loath food and digest with difficulty; their natural colour disappears and changes to a leaden hue; they have a difficulty of comprehension on account of torpor of mind and of sense; they are dull of hearing, are affected with ringing in the ears, or a confused sound in the head; the tongue is unable to do its office, either on account of the nature of the disease, or from injuries which it may have received in the paroxysms; they are agitated by convulsions; and sometimes the mind is so disturbed by the complaint, that persons labouring under it become quite fatuous or idiotic. He describes these wretched sufferers, dragging on a miserable existence, in pain and ignominy; and sometimes by the violence of the disorder, driven even to madness. " p. 25.

The Treatise contains five chapters on the history, the dissections, its causes, the diagnosis and prognosis, and, finally, the treatment or method of cure. The first chapter on its History or symptomatology, as we have just observed, is unexceptionable.

That on the Dissections is full, though not altogether complete, and in many respects loose and vague. It would have been an object well worthy of his attention, had he endeavoured to settle which of all the innumerable appearances noted by authors on epilepsy, have really any connexion whatever with the disease, and likewise which of them are its forerunners or pathological causes, and which merely its consequences. The task would be a difficult one, no doubt; but something more is called for in a work of this nature, than a simple enumeration of supposed appearances, many of which, it is well known, are not morbid, and many others the effect, not the cause, of the disorder. We shall illustrate this stricture by an example. At one place, Dr Cooke remarks, that in the opinion of Bonetus, Morgagni, Lieutaud, Tissot, and many others, the most common morbid appearance is that of serum, effused in various parts of the brain;—and in another place he says, that Mr Mansford, a very recent author on the subject, believes vascular turgescence to be universally present, and likewise to constitute the morbid cause. Now, neither of these appearances can be accounted morbid, at least in the generality of instances. For, first, as to vascular turgescence, every physiologist knows, that it is merely the consequence of the mode in which epilepsy proves fatal; a mode which is common to it with many other diseases. And, secondly, regarding serous effusion, no pathological anatomist should be ignorant, that this may exist throughout the brain so frequently and to such an extent, when no cerebral affection preceded death, that unless some other test of disease be observed, the serous effusion alone can never be assumed as morbid; and consequently, that for want of such a test, an almost incredible proportion of the facts collected both by the ancients, and even by the most recent of the moderns, is utterly useless for the purposes of science. It is by having omitted to estimate fairly these and many other similar facts, that Dr Cooke appears to us to have failed most in his account of the morbid appearances after Epilepsy.

The next chapter on the Causes of Epilepsy, treats first of the predisposing, then of the exciting causes, and lastly of the proximate cause. The grand objects of an inquiry into the predisposing and more particularly the exciting causes of a disease, are in the first place to enable us to deduce practical hints for regulating its Prophylaxis; and, secondly, to contribute to the formation and correction of our ideas regarding its pathological cause or real essence. In reference to the latter of these objects, it may be worth while to draw out a short and general account of the causes of Epilepsy; because we conceive, that a careful consideration of them may tend to throw light on some circumstances connected with this disease, not unknown perhaps to most of our readers, yet nevertheless too much overlooked by some recent inquirers on the subject.

We have good evidence for believing that Epilepsy depends directly on a peculiar affection of the brain, or of this organ conjunctly with the spine. But though various hypotheses have been started concerning the nature of that affection, it is still absolutely unknown. It may be induced by various operating powers, some of them existing within the head, others throughout the rest of the body. Of the causes existing within the head, or producing their impression there, some are apparent; of which the most remarkable are tumours of the skull and dura mater, and tubercles, abscesses, and other unnatural formations developed in the brain itself. Others are not apparent, such as the acuter passions, and more particularly a depraved state of the blood, depending on the presence of poisonous ingredients. Arsenic has frequently been known to produce true epilepsy, that is. frequent paroxysms of convulsions and sopor; lead has likewise occasionally seemed to produce it; and many vegetable poisons, though they do not cause more than one paroxysm, yet produce symptoms precisely the same with those of the best marked fit of epilepsy. Of the causes existing or producing their impression in other parts of the body, some are thought to operate by giving rise to congestion of blood in the head; but much more commonly they merely produce a peculiar impression on the sentient extremities or on the course of the nerves; which impression is then transmitted to their origin. Of these causes some are apparent, such as worms in the intestines, various irritating substances in the stomach, and irritants lying in the course of the nerves. * Other irritating causes however are not apparent,

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* Cases of this last description are not common; yet perhaps they might be met with more frequently, if dissections were always conducted with diligence. The following is one of the most remarkable instances of the kind we have read. Our readers will probably excuse us for giving an abstract of it, as it only exists in a work little known, and almost unattainable in this country. A stout young woman was one day suddenly seized with acute pain in the back of the left thigh, and torpor of the limb. Both of these affections soon disappeared; but a few months afterwards, on hitting the part, she was again seized with the same pain, and it was now followed by convulsions of the same side of 15 minutes' duration. During the next twenty months she had repeated paroxysms, which became steadily more and more frequent, severe, and lasting, and were in every respect the same as those of the truest epilepsy. At last, when she was very much reduced by the incessant fits, she was seized with one more violent than any that had gone before, and she expired a-

as in those remarkable examples, in which the epileptic fit is preceded by a sensation of a current of air or water, or a sense of creeping, beginning somewhere in the extremities, and proceeding onwards to the brain. We see, therefore, that many of the causes of epilepsy, whether impressing the brain directly or indirectly, are not recognisable by the senses. But not only are they not apparent in themselves; some of them are equally inappreciable by their organic effects. With regard to poisons, this is particularly evident, because our knowledge of them and their effects is precise. Many of those poisons which cause death by exciting an epileptic paroxysm, or something altogether resembling a fatal fit of epilepsy, produce no sign whatever, by which we could pronounce in the dead body that such a cause had operated. Even mere congestion is not recognisable either in the dead or in the living body. The fair inference is, that in like manner other causes also may operate without any obvious derangement whatever of the nervous system, even without cerebral congestion; and indeed it is highly probable, that such is the case with the passions of the mind, with worms and other intestinal irritations, and with the singular unknown causes, whose operation is attended with the *aura epileptica.* We need not be surprised, therefore, when we hear of the bodies of epileptics being frequently examined without exhibiting any morbid appearance. The want of success in these cases has been generally ascribed to negligent examination, and undoubtedly sometimes with justice. Yet the view we have now given of the Causes, will supply sufficient proof that negligence has not been always the cause of failure. Hence we fear the conclusion is inevitable, that the true pathology of epilepsy is far removed beyond the knife and eye of the anatomist. A distinguished pathologist has frankly avowed, that "the labours of pathological anatomy have hitherto shed not a ray of light on the immediate seat of epilepsy." We should be most unwilling to damp the ardour of pathologists; yet it must be feared, that, had M. Esquirol considered the evidence deducible from what has been already done on this subject, he must have added, that the labours of pathological anatomy must be very long, perhaps for ever unavailing. We are not ignorant, that according to the researches of the Wenzels, some derangement will be found in every case in the ce-

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poplectic after enduring the most horrible convulsions for thirty-three hours. On dissection, a rough, irregular, bony tumour, as large as a nut (*nocciolo*) was found lying on a branch of the sciatic nerve, in the semi-membranous muscle, at the lower third of its course. It could be felt with the finger applied on the integuments. See *Memorie della Societa' Medica di Genova.* 1801. I. 89.

rebellum or its appendages. But many other anatomists have failed to detect the appearances they notice; and this failure cannot be always ascribed, as some would have it, to negligence.

The next Chapter of Dr Cooke's work treats of the Diagnosis and Prognosis. "The diagnosis of epilepsy," he remarks, is very clear. We are in no danger of confounding it with any other disease, unless it be hysteria, from which it may be distinguished by the foaming at the mouth, the gnashing of the teeth, the blackness of the countenance, and the terrific symptoms above detailed in its history, together with the speedy termination of the fit in sleep, and the absence of the usual symptoms of hysteria, such as the globus hystericus, the palpitation of the heart, the involuntary laughing or weeping, and the other symptoms generally described in the histories of that disease." No difficulty, we apprehend, could ever be felt in distinguishing betwixt such a case of aggravated epilepsy as the author describes, and almost any form of the hysteric paroxysm. Yet it is well known, that the epileptic paroxysm is very often exceedingly mild and partial; and that, on the other hand, the hysteric paroxysm often resembles such mild cases exactly. There is no malady, in fact, whose symptoms may not be accurately assumed by this proteus-like disorder; and we apprehend, that, for distinguishing it from epilepsy, as well as from all other diseases which it imitates, much more reliance is to be placed on the presence or absence of the peculiar symptoms of hysteria, than on any nice shades of difference in its more common characters.

The last Chapter, on the Treatment of Epilepsy, occupies, as we have formerly hinted, 140 pages, or seven-twelfths of the whole book. The disproportionate length of this part of it seems to have arisen, in the first place, from the author having pointedly intended his work for a practical one; and, secondly, from his believing, that since the remedies are all of doubtful or precarious efficacy, it was necessary to make the reader acquainted with the opinions of our chief physicians concerning them.

He very properly divides the treatment into two parts, that employed against the Idiopathic disease, and that which is applicable to the Symptomatic kinds only. He sets out with an account of the means to be resorted to for averting a threatened paroxysm. These are depletion, stimulant clysters, purgatives, especially in children, emetics, opium, and pressure applied on the nerves betwixt the brain and the point where the *aura* begins. Dr Cooke is disposed to place little reliance on any of these precautions. Emetics he altogether condemns; and he even questions the utility of blood-letting as a prophylactic. During the paroxysm, the interference of the physician, he seems to think, should be confined to arranging properly the dress and position of the patient, and guarding against any injury he might inflict on himself. Stimulants are very properly discarded altogether; and bleeding he believes to be seldom useful, and often prejudicial.† For the radical cure of epilepsy, "the indications are, to correct the predisposition to the return of the fit, and to lessen or remove the exciting causes." On the presumption of the disease depending, at least in part, on plethora, many of the most esteemed authors recommend frequent bloodletting, purging, emetics, blisters, issues, restoration of suppressed discharges, restricted diet, and regulated exercise. Without a doubt, much may be effected by these means, and especially by the combination of some habitual discharge with regulated diet and regimen. As to bloodletting, it appears by the consent of most modern authorities to be unadvisable, except when the physician is controlled in the use of other means, or the patient is liable to periodical returns of plethora. Again, on the presumption that the disease sometimes depends on debility, or at least is aggravated by it, many practitioners have advised the adoption of the tonic system of treatment. Under this head, Dr Cooke has collected a formidable list of most incongruous remedies: First, he notices the cold bath, generous diet, regular exercise,—tonics unquestionably; and then adds to these the emotion of fear! Next, under the article of Tonic Medicines he arranges cinchona bark, orange-tree leaves, extract of narcissus, and the missel-toe, and the various preparations of silver, zinc, copper, lead, arsenic, antimony and mercury. Upon what fair pretext these metallic preparations are to be accounted tonics, we know not. Why antimony, lead, and arsenic in particular, are to be accounted tonics, if given to an epileptic, while in all other persons whatsoever, and even in epileptics themselves, they invariably produce the greatest debility, it is truly no easy matter to imagine. We shall not presume, however, to enter at present on the subject of their mode of action. The medical world must first be satisfied that they really are good and efficient remedies; and of this we beg still to entertain serious doubts, notwithstanding the host of authorities which

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† We have received from a Correspondent in Dublin, F. M., the notice of a case, which he thinks proved fatal in consequence of free depletion during a paroxysm. The patient, an emaciated gentleman, 45 years old, and long liable to epilepsy, was bled by a young practitioner to the extent of a quart, and died in the course of the ensuing night. Although we should not venture to recommend such strong measures in a similar case, we should, at the same time, desire to see some closer bond of connexion betwixt the effect and its alleged cause, than that which seems to weigh most powerfully with our Correspondent, to wit, the youth of the practitioner.

Dr Cooke has quoted in their favour. To the remedies just mentioned, is added an equal number of Antispasmodics, which, we apprehend, are of much more doubtful efficacy. The oil of Turpentine, which has undoubtedly been sometimes useful, should have been arranged among the remedies for symptomatic epilepsy.

The *second* indication for the cure of epilepsy, is to lessen or remove the exciting causes. We do not altogether comprehend Dr Cooke's principle of arrangement in discussing this branch of the treatment; for the only mode he mentions of removing the exciting causes, is the application of the actual cautery to the head; a measure which, we presume, ought to be ranked with bloodletting, blisters and issues, among the means for correcting the paroxysmal predisposition.

His observations on the cure of Symptomatic Epilepsy appear to us to unfold pretty distinctly the cause of the discrepancy of opinion among physicians as to the efficacy of remedies in this disease. There is much reason to believe, that, in announcing the cure of cases by particular remedies, too little attention has been paid to its important division into two species, the symptomatic and idiopathic. The two species have been very often most unjustifiably confounded together; nay, even our author himself, though he pointedly distinguishes them in most parts of his work, sometimes evidently mistakes the one for the other. This is one of the grand inconveniences of the symptomatological mode of arranging and defining diseases. Perhaps no better instance can be adduced than the present, of the inevitable tendency of this principle of arrangement to introduce confusion and error.

V.

Practical Observations on the Treatment and Cure of several varieties of Pulmonary Consumption, and on the Effects of the Vapour of Boiling Tar in that Disease. By SIR ALEXANDER CRICHTON, M.D. F.R.S. Physician in Ordinary to their Imperial Majesties the Emperor and Dowager Empress of Russia, and to his Royal Highness the Duke of Cambridge, Knight Grand Cross of the Second Order of St Vladimir, Knight of the Red Eagle of Prussia of the Second Class, &c. London, 1823. pp. 261.

THE imperfections of practical medicine, or of what is named Therapeutics, have been long the subject of complaint among physicians; but there is no family of diseases in which these imperfections are so conspicuous, as in those which consist in slow and tedious, but certain processes of disorganization. In proportion as any malady is little under the control of the healing art, the modes of treatment are manifold, and the remedies are numerous; but in the same proportion they are generally inert and unavailing; and while practical authors give elaborate and diffuse, and sometimes tedious accounts of the success of many different remedies or opposite modes of treatment, the practitioner too often finds them all equally unsuccessful, and passes from the extreme of credulity and confidence, to that of scepticism and indecision. These evils are no where felt with greater force than in the management of consumption, which many consider as totally incurable, while others are daily occupied in proposing new means of cure, and in adducing proofs of the efficacy of therapeutic measures, the success of which their own skill or more fortunate experience has enabled them to verify. While our knowledge of the pathological characters of consumption and consumptive diseases has been rendered more precise and satisfactory than formerly, by the researches of many successive inquirers, it has been the fortune of the profession to see equal activity in introducing new remedies, and improving the mode of treatment. The compass of a few years has witnessed sundry efforts and proposals to cure consumption by means of the vapour of boiling tar, by the exhibition of hydrocyanic acid, of iodine or its preparations, by long-continued squeamishness, or the pustular inflammation occasioned by rubbing the skin with tartar-emetic ointment. It is well known that the strongest testimonies have been adduced by their respective advocates in favour of the efficacy of each of these modes of management; and if it were possible to credit the results reported, we ought to expect, that if consumption were not rendered completely amenable to medical skill, the mortality occasioned by it ought to be greatly diminished indeed. Yet, though phthisical patients are ordered to live in an atmosphere of tar-vapour, or to swallow prussic acid or hydroiodate of potass in quantities, or are kept on long courses of squeamishness and sickening, or of cutaneous inflammation, the tomb is not defrauded of his prey, and the disease slays his victims with the same remorseless regularity, which he formerly observed when resisted by none of these hostile measures. This disappointment is not new in the history of physic; for the same, or similar cures, were said to be effected in the days of foxglove and the pneumatic medicine; and we all are now aware of the true value and real efficacy of these celebrated remedies. In what manner, therefore, is the difference of result to be explained? Did the remedies to which we allude, really effect a cure in the hands of those who furnished them, and fail only when exhibited by others? Was foxglove and the hydrocarbonic gas obedient to the will of Darwin, Magennis, Withering and Beddoes, yet ineffectual with their successors? Are tar-vapour, hydro-cyanic acid and iodine compliant to the commands of Crichton, Magendie, Grenville and Hufeland, yet refractory under the management of other physicians? Or have these eminent persons been deceived in the nature of the disorder, and the true powers of the remedies? Was it actually consumption which was thus treated and thus cured? We have already attempted, in a former article, to point out the fallacies with which every inquiry of this kind is beset, and to show how insufficient is the evidence hitherto adduced, to demonstrate the efficacy of remedies in controlling the disease. The same exterior signs often arise, we have seen, from different morbid processes, all of which are neither equally nor necessarily fatal; and those who have introduced new remedies, and recommended particular modes of treatment, have not always ascertained with precision the peculiar pathological state of the lung in those cases, from which they have formed their conclusions. Little reliance therefore, can be reposed in the virtues of remedies which have acquired reputation on grounds so liable to error and fallacy; and it cannot be regarded as an extreme degree of incredulity, to ascribe to them no other antiphthisical power than what resides in their property of moderating local inflammation, and in the restraints under which patients submitted to their action are necessarily placed. We will not, at the very first, put the same construction on the efficacy of the remedies which have been recently brought into public notice for the treatment of consumption, nor will we assert at once, that tar vapour, hydro-cyanic acid and iodine are totally useless or highly beneficial. This mode of prejudging a scientific question would be the more unseasonable, that stronger testimonies of the efficacy of these remedies are daily produced by their respective advocates, while trials instituted by other physicians are equally conclusive against their power. As it would be therefore equally unjust and unphilosophical to admit in their fullest extent, or deny absolutely all the virtues ascribed to these remedies without examination of the circumstances or proofs, on which their reputed efficacy is founded, it is requisite to inquire deliberately into the merits of those agents which are now recommended to the attention of physicians by the strong claims of practical experience. It was originally our intention to have submitted to the consideration of our readers, the merits and defects of the three remedies which have been lately added to the pharmacological lists, as means to aid the physician in the treatment of consumption. We find, however, that this would occupy a greater proportion of our attention than we can at present spare; and we confine our observations to the subject of Sir A. Crichton's work,—the merits of tar vapour as a remedy in consumption, and the evidence on which its recommendation is founded.

We feel ourselves compelled, in the first place, with regret to say, that the work is not entitled to much praise, either for novelty of information, or excellence in matter or performance. Sir A. Crichton does not confine his work strictly to examination or description of the sanative powers of tar-vapour, and its application in the cure of consumption; but has entered into details more or less extensive, on many points connected with the formation and pathology of the disease. He has thus examined, in separate chapters, the influence of temperature and climate in preventing and curing consumption, the prevention and treatment of tubercular consumption, consumption proceeding from hæmoptysis, neglected peripneumony, bronchitis and ulceration of the larynx, and the questions of the hereditary and contagious nature of the disease. These several subjects occupy no fewer than nine chapters, from the second to the ninth inclusive, for the third is given twice over, though on a different subject;—and in this manner constitute by far the greatest part of the volume. We shall not positively assert that the information communicated by our author on these subjects is useless or superfluous; since it may be presumed that a physician, with the years and experience of Sir Alexander Crichton must have acquired much valuable matter, and could scarcely be expected to write on any subject without improvement to his readers. But the observations of our author on all the points above enumerated, have more the appearance of being disposed in their present situation for the purpose of making a respectable volume on consumption and its treatment, than for proving the true merits of the remedy which he so strenuously recommends. Nor are they, it must be observed, the result of his own personal observation, but collected from several sources in which his readers might, with equal facility, and we think as much benefit, have studied them at their own leisure. Now, though we do not pretend to dictate to authors, or to suggest to them the form and the kind of work which they ought to publish; we, however, conceive, that there is a sort of understood agreement between every author of practical observations and his readers, that the matter with which he proposes to instruct them, is to be either entirely or very completely the result of personal experience and observation. So little is this obvious principle of natural justice respected in the volume before us, that we venture to assert, without fear of contradiction, that the whole of the matter which this literary Knight can reasonably claim as his own personal property, may be compressed, without injury either to author or reader, into a single chapter not larger than any one of the eleven which form the paper and letter-press of this elegant octavo. The natural effect of this violation of justice in the duty of authorship, is, that these "Practical Observations" contain little or no practical instruction not already well known; and must be regarded as one of the numerous works, of which the title announces any thing but the real purpose,—or answers as a sort of passport to the desultory and unconnected mode of writing, which, under the garb of practice and experience, sanctions a multitude of useless *verbiage.* The short sketches which are intended for pathological inquiries, are taken at second-hand from works, which the reader will find more interest in studying directly; and the instructions which the author delivers as therapeutic rules, are disposed in an order so confused and irregular, that if they possessed any merit in themselves, which in our judgment is doubtful, it is completely destroyed by the desultory and unprofitable mode in which it is communicated. These considerations would have induced us to pass in silence a book which possesses no requisite deserving of attention; and gladly would we have spared ourselves the pain of animadversion, where it must be often accompanied with censure. But the interest of the subject, and the obvious necessity of inquiring into the merits of Sir Alexander Crichton's therapeutic instructions, counterbalance the disadvantages of time lost and space misapplied; and we are not unwilling to sacrifice our private feelings, for the purpose of examining a question, in which not only the practical utility of the profession is concerned, but in which the feelings of perhaps nine-tenths of the population of this country are in a greater or less degree interested. We shall, however, for reasons which the reader may easily conjecture from what we have said above, confine our present remarks very closely to the use and sanative powers of Sir Alexander Crichton's favourite remedy.

The author commences his work with extracts from Laennec, to demonstrate the sanability of tubercular consumption; and appears not a little triumphant in obtaining this forcible argument, at the very outset, to silence the lips of the malecontents, and win the confidence of unbelievers. We shall show the force of this mode of argumentation very soon. The author then proceeds, somewhat abruptly, to substantiate his claim to originality in the employment of tar vapour, against the encroachments of a Dr Lazaretto of Portsea, who is charged with a violation of the rights of authorship and patenteeism, in announcing himself to the world as the distinguished inventor of this method of cure. This *parvenu*, it appears, asserts, that the fortunate discovery was, like all other good things in science, and especially in medicine, the offspring of accident, and is so modest as to claim merely the merit of applying it in a scientific manner. With this, of course, Sir Alexander is grievously offended, and declaims against the uncandid behaviour of Dr Lazaretto, in the language of a discoverer defrauded of his well-earned celebrity. Now, we have little difficulty in saying, that we believe Sir Alexander Crichton to be the first physician who made any thing like what is called a *sensation* about tar vapour. In other words, he is assuredly entitled to the undivided honour of writing, and speaking, and making a bruit about the employment of it in treating consumption; and the physician of Portsea must come in for a secondary portion only of that renown, which awaits those who introduce new remedies to public notice.

But although Sir Alexander be allowed this degree of merit, it will appear, on examination, to diminish to a very slender pittance indeed; and it will prove to be of that fragile and inferior nature, that it may be doubtful how far it is worth possession. And as our author has shown in his Preface, that he is not entirely ignorant of the history of the employment of vapours in the treatment of pulmonary disorders, we think it so much more singular that a physician, whose reputation should depend on more substantial materials, should attach to it the importance which it is evident he does. Our readers will understand us more distinctly, by attention to the following brief sketch.

The idea of a local application to the surface of the lungs, inspired with the air in the treatment of pulmonary complaints, appears to have taken early possession of the minds of physicians; but our countryman Christopher Bennet may be mentioned as the first who thought of reducing it to system, and delivering rules for its management, as a branch of therapeutic medicine. This antique and somewhat original worthy, who flourished about the middle of the seventeenth century, published two small treatises, the "*Theatrum Tabidorum*," and the "*Vestibulum Tabidorum*," in which, among other therapeutic instructions, he gave regular formulæ for fumigations (*suffitus*), and inhalations (*halitus*), in various forms of consumptive disease. The former, in which only we are now interested, were produced by burning slowly, and by gentle heat, frankincence, turpentine, styrax, or other balsamic or resinous substances, combined sometimes with aromatic medicines. With the vapours thus formed, the chamber of the patient was to be filled, the doors and windows being previously shut; and they were in this manner ordered to be inhaled with the air inspired. This method of treatment appears never to have come into very general use in this country, and was certainly after some time completely forgotten; for it has been remarked that Morton, who wrote on consumption in 1689, and his contemporary Leigh, who published his *Phthisiologia Lancastriensis* in 1694, though they enumerate the usual remedies and methods of treatment, nowhere mention the practice of fumigations. They were afterwards, however, noticed in a modified form by Willis and Fuller, and were warmly recommended by Dr Mead. But it was so late as the year 1770, * before any very satisfactory trials were made, and before instructive results were laid before the profession.

About this time, M. Billard, Surgeon-major to the Royal Corps of Artillery and Infantry of Marine at Brest, brought the subject before the Academy of Surgery, under the following circumstances. A soldier under his care, after a violent peripneumony, became affected with the usual phenomena of consumption; and, after lingering for seven months in various hospitals in Brest and its vicinity, seemed to be drawing near his end in the month of November 1770. By singular good fortune, says the narrator, the Chevalier de Fautras d'Andreuil, Major of Brigade, who saw the man at his return to the Hospital of Brest, had, some days before, in an affection of the throat, with considerable expectoration, derived marked relief from the vapour of a mixture of yellow wax and resin, with which he had been sealing some bottles in his cellar. The Chevalier de Fautras, with a keen interest in the fate of the dying soldier, instantly waited on M. Billard, and proposed to take the sole charge of his patient, with a confident and enthusiastic assurance that a cure would be effected, by respiring the same vapour from which he had himself derived so much benefit. M. Billard complied with this very natural request, and the soldier was forthwith lodged and boarded, at the charge of this officer, in a small apartment in the Gallery of the Barrack Pavilions, but still under the professional attentions of his surgeon, M. Billard. With a restrained diet, composed of milk, ptisans, and the farinaceous vegetables, this soldier began, on the 9th of November, to breathe a vapour produced from a mixture of one pound of recent yellow wax unpurified, and an equal quantity of resin, melted in a glazed earthen vessel on a stove placed in the centre of his apartment. The first application of this vapour was attended with more ease in breathing, than the patient had for some time experienced. On the fifth day he began to feel relief, and his cough was less fatiguing. About the end of the month (three weeks), the improvement was conspicuous; the expectoration was better, the cough less frequent, and the fever much abated; and M. B. began to entertain hopes of a favourable termination. In the course of December the patient was allowed to go out; and by the end of the month, when his health was much reestablished, he discontinued the fumigations. A violent accession of fever from exposure to cold in the month of January 1771, seemed to threaten him with a relapse; but it was gone the following day; and in a few days more he was in condition to resume his duty, when he sailed, first for St Domingo, and afterwards to the Mediterranean, and enjoyed good health for many years afterwards.

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* Mem. de l'Acad. de Chirurgie, Tom. v. p. 549, & 561.

The favourable result of this experiment, which is the one mentioned in Baron Grimm's correspondence, led M. Billard to try his remedy on other patients with phthisical symptoms; and he reported to the Academy the details of several cases in which, with one exception, it had been attended with the same fortunate event.

After Billard, Francis Home of Edinburgh,* and John Mudge of Liverpool, are the first who notice the use of this application in consumption. The latter, who, in early life, had been brought to the brink of the grave with phthisical symptoms, and was indebted for recovery to the use of a large issue between his shoulders, and living on asses milk for a whole year, mentions, in his "Radical Cure for a Catarrhous Cough," published in the year 1782, the case of a man whom he believed to have suppuration of his lungs, from the quantity of fetid matter which he spit up. "The patient," says he, "was ordered to sit in a very small room, which was filled with the smoke of the common resin, by now and then sprinkling a little of it upon a hot iron; and this he continued to do twice a day for a fortnight. The patient recovered perfectly." †

These testimonies, it may be argued, do not prove any thing regarding the adoption of the identical remedy of Sir A. Crichton. We therefore proceed to say, that Dr Gilchrist, so long ago as 1771, which was previous both to Mudge's publication, and likewise to that of Billard's cases, had the sagacity, in his Treatise on the Use of Sea Voyages in Medicine, to suggest the internal use of tar as equally efficacious, and more accessible than any of the more costly balsamic medicines;* and though it cannot be regarded as a difficult transition, to pass from the internal exhibition of tar thus associated with balsamic medicines, to its local application in vapour as *they* had been employed; we must, however, inform Sir Alexander Crichton and our readers, that Dr Gilchrist not only recommended the vapour issuing from tar and hot water, as a "cooling, resolving, antiseptic and detergent remedy," but has actually recorded a case of consumption which recovered under the use of "tar pills with other detergents, and the steam of tar dropped upon hot water."† So early also as 1787, we have the positive testimony of Dr Rush, that it was thus employed in America; for "I have seen," says this writer, "the pain in the breast relieved by receiving the vapour from a mixture of equal parts of tar, bran, and boiling water, into the lungs." ‖ It would not be difficult to multiply proofs of this description, to show that the vapour of tar, as a fumigation or inhaled remedy, was by no means unknown in the treatment of consumption; but as this is at once foreign to our purpose, and would occupy more leisure than we can at present allot to it, we conclude this digression by quoting the following short but decisive case, which is recorded in the Reports of Dr Beddoes, on the effects of factitious airs,—a work published so long ago as the year 1795.

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* Clinical Experiments, 1780, p. 119. Vapour of *Thus* (frankincense) in Consumption.

† A Radical Cure for a Catarrhous Cough, &c. by John Mudge, F.R.S. Lond. 1782, p. 74.

"A young lady, labouring under every symptom of confirmed *phthisis pulmonalis,* and daily sinking under the disease, happened to be residing at the house of an eminent tar-merchant, to whom she was related. No remedy seeming in the least degree efficacious, it was proposed that she should walk in one of his warehouses, where a large quantity of plantation-tar was usually kept. The first time she was introduced into it was on a Monday morning, when it was imagined, in consequence of the warehouse having been shut up since the Saturday afternoon, the air would be the most fully impregnated with effluvia. She walked a considerable time through the different ranges of barrels, and bore the experiment very well. This practice was persisted in several mornings with advantage; and finding the cough and other symptoms gradually decrease, she persevered till she was restored to perfect health." §

Now, it is a matter of utter indifference whether the tar vapour cured this young lady, or even whether the narration were a complete fiction; for though either supposition be admitted, the simple story is sufficient to show, that there was at that time a belief more or less general, that air impregnated with the vapour or effluvium of tar might be inhaled into the lungs of consumptive patients with advantage. In what important circumstance, therefore, does the merit of Sir Alexander Crichton consist, or on what improvement or novelty does he found his claims of originality? Is it for the mere application of heat, or for the construction of an apparatus with an appropriate lamp, that he insists on public attention to his claims? If this be the sole merit of this eminent physician, we humbly conceive that it is the most paltry and unsubstantial object of attention to any individual whatever, and infinitely beneath the dignity of a Knight Grand Cross of the order of St Vladimir.

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* The Use of Sea Voyages in Medicine, and particularly in Consumption, by Ebenezer Gilchrist, M.D. p. 129.

† Ibid. Supplement, pp. 250 & 259.

‖ Causes and Cure of Pulmonary Consumption,—in Medical Inquiries. By Benjamin Rush, M. D. Vol. ii. p. 118.

§ Considerations on the Medicinal Use, and on the Production of Factitious Airs. Part I. By Thomas Beddoes, M. D., &c. 1795. p. 165.

Since, therefore, the originality or inventive genius of Sir Alexander Crichton depends on a foundation so precarious, it may be reasonably expected that his practical observations should furnish more substantial and less questionable matter of self-gratulation; and we avow, that it gives us much pleasure to observe, that he has not recommended his remedy indiscriminately in all pulmonary and consumptive disorders, but attempts to confine it more particularly to certain forms of the disease. He thus condemns its employment in acute hæmoptysis (pulmonary apoplexy), and wherever there are signs of acute inflammation; and assures his reader that no benefit can be expected from inhalation of tar vapour in cases of rapid consumptive disease, which depend on the speedy suppuration of many successive tubercles in the same lung.

" It may be remarked, that as by far the greater number of consumptive cases which occur in Great Britain are of this acute and destructive kind, the vapour of tar, should it have been solely trusted to, must have failed; for such cases, even where a little hope is to be entertained, demand the cooperation of internal remedies: and if we consider the nature of our climate, the construction of our hospitals and houses, there is little reason for wonder at the failure." p. 21.

With this we entirely approve; but cannot agree with the ingenious Knight in the opinion delivered in the first member of the following paragraph.

" But the greatest benefit in tubercular phthisis, when the tubercles are softening, and are not too numerous or extensive, is to be derived from breathing the vapour of boiling tar, from temperature, a judicious diet, corresponding to the peculiarities of the patient's stomach and constitution, and also to the uninterrupted employment of antiscrophulous remedies. Where the pulse is hard or full, the skin hot and dry, and the expectoration scanty, I do not employ the tar vapour." p. 108.

Now, so far should we be from employing the tar-vapour in such circumstances, that we have no hesitation in saying that it can do no positive good, and will very probably do harm, in persons with tuberculated lungs. The truth is, that Sir Alexander Crichton has allowed himself to be entirely misled in his therapeutic theories by the results of M. Laennec's observations on the discharge of tubercular matter. Though there can be no doubt of the actuality of this discharge, and the cicatrization which in a few rare cases succeeds, it is quite absurd and ridiculous to suppose that the inhalation of tar-vapour can have any good effect in promoting it. If any reason can be assigned for the use of this remedy in such circumstances, it is to be found in the secondary chronic bronchial inflammation which attends the process of tubercular destruction. * Yet we doubt whether the advantage derived in this manner is not completely overbalanced by the probable injury occasioned in an organ already too much irritated by extensive disorganization. Is tar-vapour equally beneficial in every stage of tubercular disease; and does it exercise the same influence in quiescent or immature tubercles, and on those which are softening, or converted into vomicæ? Is Sir Alexander always ready to determine, in any given case, the precise progress of disorganization, and to employ or withhold his remedy accordingly? We do not deny that tar-vapour is occasionally useful in pneumonic diseases, especially where there is inflammation of the bronchial membrane; but we are satisfied, that neither principle nor experience sanctions its adoption in the treatment of tubercular consumption.

What does Sir A. Crichton mean by talking of a whole lobe of the lungs reduced to pus, where no expectoration of the fluid had occurred? (p. 158). Could his acquaintance with M. Laennec, who forms the great pillar of his therapeutic principles, not inform him that this was a pathological impossibility, and that, instead of being regarded as suppurations of the lung, such instances are examples of chronic pleurisy?

Our author shows more reason in recommending his remedy in the last stage of peripneumony, and in suppuration of the pulmonic tissue; and if it were always possible to distinguish these states of the lung from tubercular deposition, active inflammation, and hæmoptysical engorgement, in which, if it produce any effect at all, it will most assuredly be injurious, the remedy is undoubtedly calculated to be beneficial.

" When the suppuration is once reestablished, the vapour of tar ought again to be had recourse to, but in a very diluted state, while

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* See our Observations on the Sanability of Consumption, and on the Process of Tubercular Destruction, at p. 195.

there is any disposition to active inflammation. The state of atony is sure to come on in the progress of ulceration: and it is better to anticipate this a little, than to lose any valuable moments by deferring, for too long a period, the use of the tar-vapour." p. 167.

After the opinion which we above expressed on the nature of the contents of this work, it will not be expected that we should enter minutely into our author's pathological disquisitions. Where they are good, we say it with confusion of face, and a sore heart for the glory of medical knighthood, they are uniformly derived from other sources; where they are indifferent, or actually erroneous, they are generally the personal property of the author; and, as such, we leave him in undisturbed possession of them.

We had, however, intended to have bestowed more minute attention on the Tenth Chapter, which is professedly appropriated to the exposition of the method of employing the tar-vapour, and the best temperature of Hospitals and Houses for the Recovery of the Consumptive; and with this view, we had prepared sundry observations, with which we doubted not, that our readers would be highly delighted and edified,—and we had further marked a variety of interesting extracts to lay before them; but time presses,—and, what is more satisfactory as an excuse, we do not wish to prevent the work of our author from yielding a fair and reasonable harvest of reward; and we conclude with a recommendation to all concerned to peruse the Tenth and Eleventh Chapters of the "Practical Observations," and thus to qualify themselves for the administration of Tar Vapour, according to the rules and precautions delivered by the author.

VI.

Transactions of the Medico-Chirurgical Society of Edinburgh. Instituted August 2d, 1821, *with Plates.* Edinburgh, printed for Adam Black and Thomas Ireland & Co., and Longman & Co., and T. and G. Underwood, London. 1824. 8vo. pp. 694.

Somewhat more than two years ago, an association of the practitioners and professional gentlemen in this city was instituted for the purpose of communicating such facts, observations and reasonings, as their several pursuits and duties suggested, and for concentrating and preserving information, which, in other circumstances, must have been dispersed in various quarters, or perhaps entirely lost. The volume before us, which is the first public result of their researches, contains a considerable number of communications on various subjects connected with physiology, pathology, and therapeutics, and may be justly said to be the depository of much useful and interesting information. It is of course unreasonable to expect the whole volume to be of equal merit; and we are unwilling to institute any comparison which might give rise, either in our own minds or in those of others, to invidious reflections. The transactions of Societies must be at once miscellaneous in their nature, unequal in the interest of their subjects and in the style of performance, and varied in all their qualities. To do them any thing like justice, therefore, would require an enumeration of the merits of each paper individually; and as this is a task quite inconvenient and unseasonable at present, our sole purpose is to notice the appearance of the work, and to glance at its general merits. At some future period, it is not unlikely that we may revert more particularly to some of those articles of which it consists. The following Table will enable our remote readers to form some idea of the nature of those subjects which have attracted the attention of the Medico-Chirurgical Society of Edinburgh, since the date of its institution in 1821.

1. Pathology of the Heart, by Dr Abercrombie. 2. Syphilitic Caries of the Cranium, by Dr Ballingall. 3. Exfoliation of Cranium by Absorption, by Professor Russell. 4. and 5. Death by Cold, with Pathological Observations on Apoplexy, &c. by Dr Kellie. 6. Dislocation of Hip and Shoulder-joints, by Dr Hunter. 7. Tobacco in Tetanus, by Dr Anderson. 8. Anæmia, by Dr Combe. 9. Hydrocephalus, with bifid Brain, by Dr Duncan junior. 10. Phrenitis, with Cerebral Congestion, by Mr Rhind. 11. Tubercular Disease of Oesophagus, &c. Dr Hay. 12. Malformation of Heart, by Dr Holmes. 13. Tuberculated Peritoneum, by Dr Moncrief. 14. Melanosis, by Messrs Cullen and Carswell. 15. Stramonium, by Dr Begbie. 16. Influence of Panic in propagating Fever, by Dr Hamilton. 17. Erosion and Perforation of Alimentary Canal, by Dr J. Gairdner. 18. Cases of Blasting with Lightning, by Dr Macaulay. 19. Pathology of Scrophulous Diseases, by Dr Alison. 20. Inflammation of Cephalic Vein, by Dr Duncan junior. 21. and 22. Diffuse Inflammation of Cellular Tissue, by Dr Duncan junior. 23. Injury of Head with supposed Counterfracture, by Mr Macfarlane. 24. Morbid Preparations, by Mr Brown. 25. Purpura, by Dr E. Gairdner. 26. Fatal Purpura, by Mr Wood. 27. Nature and Origin of Tubercular Diseases, by Dr Abercrombie.

In venturing to deliver any opinion on the merits of these papers, we feel the awkwardness and difficulty of speaking of an Institution, which must be regarded as connected with the Edinburgh School of Medicine, and with which we feel ourselves in some degree identified. To express ourselves in plain terms, we are not unaware that any commendation bestowed by us, may be ascribed to mere partiality and affection for the medical character of the city in which we received our education. We presume, however, that most of our readers will agree with us in thinking, that the communications of Drs Abercrombie, Kellie, Duncan junior, and Alison, are unquestionably the most interesting; and we farther trust, that the reputation of these gentlemen will secure to them this distinction, without giving rise to any other feeling among their fellow-contributors, than that of honourable emulation. The article of Dr Kellie abounds with ingenious and novel, but perhaps too theoretical views of the cerebral circulation. The 9th article, by Dr Duncan, illustrates a curious point in the general anatomy and mode of formation of the brain. The 21st and 22d, by the same gentleman, contains a full and interesting account, pathological and practical, of a disease very imperfectly known, and to which little or no attention has hitherto been given. The paper of Dr Alison should, in our opinion, be generally known among practitioners in every part of the kingdom, as it relates to a subject in medical police most important to this country, and which can be investigated with success only by the united efforts of many inquirers. Besides these papers, there are others of merit and interest by no means inconsiderable. Among these we may mention Dr Combe's Case of Anæmia, Dr Hay's Case of Thoracic Abscess, Messrs Cullen and Carsewell's Essay on Melanosis, Dr Gairdner's Observations on Erosions of the Alimentary Canal, and Dr Macaulay's Account of the Effects of Lightning on the Human Body. Mr William Wood's Case of Fatal Purpura, which, with an excess of modesty, is thrust into a secret nook of the volume, is, in our judgment, a most valuable example of the disease, and throws more light on its pathology than has been done by almost all that has been yet written on it. †

There is one observation only, for which we know that the Medico-Chirurgical Society will not be displeased with us. We think we can discover, on perusing the volume, a sort of lurking fear of offending contributors in the selection of articles for publication; and it is evident, that there are a few papers which are indebted to this feeling for the place which they occupy in the Transactions of the Society. Now, though it is undeniable, that no Society is understood to pledge itself either for the accuracy or the general merit of any communication, and every contributor is responsible for the individual excellences or sins of his writings, yet it is well known, that good papers confer honour on their authors, while, of those which are indifferent, a portion of the discredit is shared by the Society. The dignity of a scientific institution, therefore, as well as the interest of the profession and the public whom they propose to benefit, requires that they should exercise a discretionary power somewhat unlimited; and that they should be influenced neither by hope nor by fear, by love, by friendship, nor by general respect in the discharge of this important duty. It is for this reason that we venture to suggest to the Society at large, and especially to its publication Committee, though with the utmost deference and respect, that in this age of writing and publication, a rigid and judicious system of selection should be adopted; and that no consideration whatever should induce them to sanction the publication of papers which are not distinguished either for illustrating some important fact, correcting those which are erroneous or doubtful, unfolding new or unknown subjects, or giving new and probable views of obscure phenomena;—or, in short, which do not contribute very directly to the improvement of the theoretical or practical parts of medicine. On this principle, we conceive, it will be the interest of the Medico-Chirurgical Society to decline the publication of all commonplace cases of disease, cases in which peculiarities are not explained by dissection, and all papers on remedies, the effects of which are vaguely or imperfectly observed. We apprehend, that, however suitable such papers as the 10th, 15th, 23d and 25th, may be as mere communications to the Society, few of those readers who are in the habit of perusing the Transactions of Scientific Societies, will approve of their admission into the present volume. The case given by Dr Ballingall presents no peculiarity to which the experience of medical practitioners could not furnish many parallels; but it is impossible to perceive, how the inference drawn by the author flows from it, and equally difficult to discover, how the administration of mercury in some cases without bad effects, can prove the mercurial treatment to be universally harmless, or generally beneficial. Surely the extensive experience of Mr Russell might have enabled him to honour the first appearance of the Transactions, with a subject more calculated to display his profound surgical knowledge than that of exfoliation of the cranial bones. Is exfoliation ever performed without the process of absorption? We regret to see that Dr Abercrombie has assumed the arduous and complicated task of an analytic chemist, in examining the properties of tubercular deposition. To the most accurate and practised chemists, the analysis of animal substances has not yet been a field of satisfactory research; and the pathological eminence of Dr Abercrombie should forbid him from risking his reputation on a subject, in which imperfect results are useless, and which requires to be illustrated by all the aids and resources of the most refined analysis.

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† See also a Case by Dr Duncan jun., in Med. and Surg. Journ. Vol. XVIII. p. 405.

With such wholesome restrictions, however, as those which we have now suggested, we are convinced that the Transactions of the Edinburgh Medico-Chirurgical Society will not only bear comparison with those of London, the great and yet unequalled model of scientific Acts, but will do infinite credit to the medical character of the city in which it is established. These sentiments are uttered in the spirit of rational and judicious affection; and we conclude "*Perpetua esto.*"

PART III.

MEDICAL INTELLIGENCE.

Biographical Sketch of the late Dr Baillie. By JAMES WARDROP, Esq. F. R. S. Edinburgh, Surgeon Extraordinary to the King.

THERE never was, perhaps, a period in which medicine was practised with more renown, than during the last thirty years in Great Britain. Whilst the rest of Europe was suffering under all the horrors of war and civil discord, a zeal for the cultivation of science was kindled in this country; and in no department of human knowledge was it more ardent, or more successfully prosecuted, than in the study of Medicine.

There has not been an individual who has held a more exalted station in the medical profession, during this period, than the late Dr Baillie; and it is hoped that the present attempt to record some of the more prominent incidents of his life, will be received as an humble, though sincere, tribute of respect to the memory of that distinguished Physician.

It is no less instructive than it is pleasing, to contemplate the life and character of men who have performed the duties of their profession with honour and ability. Their example has a powerful influence on the human mind; and Biography becomes of great importance to mankind, when it can hold up for imitation the example of those who have been distinguished as much for their moral worth as their intellectual endowments. Indeed, in no department of civilized life can the moral character be so justly appreciated, as in the medical profession. In the exercise of his duties, so much depends on the honour and integrity of the physician, that, when possessed of those high endowments, he is deservedly entitled to public gratitude and respect; while his character cannot be too highly estimated by his professional brethren.

Dr Mathew Baillie was a native of Scotland: he was born in the year 1767, in the manse of Shots, in the county of Lanark, where his father was the clergyman; a spot well known to the writer of these pages; and, though remarkable only for the barrenness of its soil, and the ungenial nature of its climate, it is now rendered worthy of the traveller's notice, as the birth-place of an illustrious man. Soon after the above period, his father was removed to the church of Hamilton, and, when his son was in his thirteenth year, was elected Professor of Divinity in the University of Glasgow. After having had all the advantages of an excellent general education in that University, and at Oxford, where he took his degree in 1787, young Baillie came to London in his nineteenth year, to commence his medical studies under the immediate superintendance of his uncles, William and John Hunter, the two most distinguished medical men then in the metropolis.

In the Museums and Dissecting-rooms of those great anatomists, was laid the foundation of all Dr Baillie's future acquirements and reputation. He soon distinguished himself as an expert and skilful anatomist, and, in two years from the commencement of his studies, became chief teacher in the anatomical theatre. He had not been thus employed more than twelve months, when Dr William Hunter died, bequeathing to his nephew, as long as he should teach anatomy, the use of that Museum, which is now deposited in the University of Glasgow, and forms so noble a monument of its Founder. It may here be remarked, as a most extraordinary circumstance, that two brothers should have collected the two most splendid and most extensive museums extant; and it is hardly possible for any person who now visits them, to conceive that either could have been the work of a single individual.

Two years after his uncle's death, Dr Baillie gave his first complete course of anatomical lectures. As a lecturer, he soon attained to great eminence, being remarkable for the simplicity and perspicuity of his demonstrations, the order and method of his style, and the clearness and distinctness of his delivery.

The taste which the Hunters created in this country for the study of Morbid Anatomy, marks the period in which they lived as one of the most important, perhaps, and proud eras in medical history; and their nephew soon acquired a taste for this branch of medicine, which he cultivated with great assiduity and success.

Surrounded by the labours of his uncles in morbid anatomy, it was not long before he made an admirable use of the valuable stores contained in their museums. A multitude of important pathological facts were there illustrated, which he subsequently arranged, and made the basis of his work on Morbid Anatomy, first published in 1795,—a work which, whether we consider the subject, or the manner in which it is treated, must be estimated as a most valuable acquisition to medical science. Till that period, all the information to be found on the subject was scattered through the writings chiefly of Bonetus and Morgagni; so that the distinct and concise account of the diseased changes of different organs given by Dr Baillie, formed not only a most accurate elementary work, but was no less useful as a general treatise. Its estimation abroad was shown from the fact of its having been translated into both French and German soon after its publication.

About four years after the appearance of "the Morbid Anatomy," the "Engravings" for its elucidation were published; and of these it may be truly said, that, for choice of subject, accuracy of drawing, and beauty of engraving, they have never been excelled; and, along with the work, must remain a lasting memorial of the zeal, the industry, and the talent of their author. To these publications may be added several papers, which appeared at intervals in the Transactions of the Royal Society, of the Society for the Improvement of Medical and Surgical Knowledge, and of the College of Physicians; in all of which are to be found some pathological facts worthy of record.

While Dr Baillie was occupied in the investigation of Morbid Anatomy, and in giving lectures, he did not fail to take every opportunity of preserving, for a private collection, specimens of diseased organs; and though small, when compared with that of either of the Hunters, yet the Museum he thus formed is highly worthy of him, and contains many excellent and well chosen specimens, illustrating almost every organic change. It may here be noticed, that, three years before his death, he gave this collection to the College of Physicians, with 600*l.* for the purpose of keeping it in a fit state of preservation; and that, by his will, he has added his medical library, containing an excellent selection both of ancient and modern works. Dr Baillie seemed strongly impressed with the importance of the study of morbid anatomy to the physician; and, in depositing his collection in that College, he indulged the hope that some of its members might be stimulated to similar pursuits.

About the year 1797, Dr Baillie was appointed one of the physicians of St George's Hospital. Till then, his opportunities of studying the practical part of his profession had been comparatively limited; but great assiduity and attention enabled him to acquire that tact in discriminating diseases which formed a striking feature in his future character; an acquirement which few attain who have not had constant opportunities, in early life, of studying diseases.

After having, for thirteen years, faithfully and assiduously performed the duties of his office in St George's Hospital, he resigned that appointment, giving up, at the same time, his anatomical lectures. The resignation of St George's Hospital was highly praiseworthy; for the medical officers of hospitals have often been severely censured for retaining such situations from unworthy motives, and thus sacrificing the original and charitable objects of such institutions to their own selfish views; but, on all occasions of this kind, Dr Baillie indulged in an honourable and just pride, and never hesitated to relinquish, for the public good, what some might have considered his own personal interest.

There is no profession, perhaps, in which the progress of even the best qualified is so slow as in the practice of physic: this necessarily arises from success depending entirely on individual exertion and assiduity; and it is a well known fact, that most of those medical men, who have had the greatest share of public confidence, have all been considerably advanced in years. This was the case, in a remarkable degree, with Dr Baillie; for, when the great celebrity of the latter years of his life is considered, it might have been expected that he would have early enjoyed no small portion of his fame. It was not, however, till he had nearly reached his fortieth year, that he found himself fairly established in private practice; but it would seem as if he had only required to be known; for, from that period, he became at once completely engaged in the practical part of his profession, and, in a very few years, rose to that eminence which is so universally acknowledged.

It is curious to trace the variety of circumstances which have led medical practitioners to eminence in the metropolis. Dr Baillie was one of the very few physicians of his day, whose success is to be attributed wholly to professional skill, adorned with the most estimable private virtues. Minute anatomical study had been too much disregarded by physicians, and conceived necessary only for those who practised surgery. The comprehensive knowledge of anatomy, therefore, which he possessed, could not fail to give him immense advantages over those who were competing with him for private practice. Whenever more than ordinary scientific precision was wanted, he was now resorted to; and the advantages which his anatomical skill afforded him, established his reputation among the better informed members of his profession.

Dr Baillie possessed, in an eminent degree, a facility in distinguishing diseases,—one of the most important qualities in the practice of medicine; for a want of accurately discriminating symptomatic from primary affections, leads to the most serious errors; whilst, when a disease is once distinctly characterised, and the peculiarities of each case defined, the cure may often be considered as half performed. Habits of attentive observation had also enabled him to know, with great accuracy, the precise extent of the powers of medicine; indeed, there was no class of cases more likely to fall under his observation than those where medicines had been abused; for younger practitioners are too apt to carry any particular system of treatment beyond its proper limits; Dr Baillie's readiness, therefore, in seeing this abuse, rendered his opinions, in many such cases, of great value.

In nothing did he excel more than in the clearness, the conciseness, and the unaffected simplicity of the mode of delivering or of writing his opinions. He had the particular merit of leaving no ambiguity in the mind of his patient; and the language he employed was so plain, and so free from scientific jargon, that I have often heard them repeat, word for word, all they had heard him say on such occasions. He possessed also, in an unusual degree, the power of simplifying and illustrating, by analogy, what appeared complex and unintelligible, and was thus able to give patients a correct and satisfactory idea of the nature of their complaint.

He had a most natural, unassuming, but decided manner, which, in the exercise of his professional duties, was the same to all persons, and on all occassions. His mind was always quietly, but eagerly directed, to the investigation of the symptoms of their disease; and he had so distinct and systematic a mode of putting questions, that the answers often presented a connected view of the whole, and could not fail to impress the patient with his clear and comprehensive knowledge.

The same liberal and just principles which, on all occasions, guided Dr Baillie's conduct in private life towards individuals, were equally remarkable in his public duties: he never countenanced any measures which had the appearance of oppression or hostility towards the members of his profession. Men seldom act collectively, with the same honour and integrity, as they would do individually; and the member of a public body requires a great share of moral courage who opposes those measures of his associates, which he may not himself approve of; but if there was one quality more than another, which gave Dr Baillie the public confidence he enjoyed, and raised him to the zenith of professional distinction, I should say that it was his inflexible integrity.

Till the very close of life, Dr Baillie continued to be completely engaged in the arduous, and, to his mind, often irksome duties of his profession, with the exception of an annual relaxation of a few months in every autumn. One of the first seasons in which he took this indulgence, he devoted to a visit to the home of his fathers, after an absence of thirty years. The love of country was a prominent feature in his character; and though his habits and avocations precluded him from contemplating a permanent residence in Scotland, yet his soul was ever awake to the welfare of his native land. The feelings which this journey awakened, proved very different from what he had allowed his imagination to paint; and I have heard him express the sorrow he experienced in witnessing the sad changes which, during so long an absence, had occurred in the small circle of his connexions, and amongst the companions of his early years; and it may easily be conceived, that a mind and heart like his, could have little satisfaction, in being, as it were, compelled to contrast his own triumphant success with the domestic misfortunes of many of his schoolfellows.

The extent of Dr Baillie's practice, and the number of years in which he was fully occupied, enabled him not only to indulge in every worldly comfort and luxury, but to amass a very considerable fortune. With feelings of proper value for his talents and rank in his profession, he was at the same time most liberal when he suspected the circumstances of the individual required it; whilst, on all occasions of generosity, and in deeds of charity, his bounty was conspicuous.

He was equally remarkable for the consideration he paid to the feelings and character of his professional brethren, more particularly to those of the younger branches, and others who did not enjoy the full confidence of the public; and nothing more strongly marked the respect and value they entertained for him, than that universal expression of sorrow amongst them, when the intelligence of his death reached the metropolis.

Dr Baillie received all those professional distinctions due to his merits. He was a member of the Royal Societies of London and Edinburgh, and of many other learned bodies ; he was the first President of the Medical and Chirurgical Society, and one of the physicians of the late King.

Such was his public life; the brief narrative of its close now only remains.

Dr Baillie's health had for many years suffered from the fatigue of business ; and it may justly be said, that he fell a victim to the constant excitement of his professional duties. His physical frame was far from being originally robust; and, as it often happened that he had more business than it was possible for him to perform, he fell gradually into a state of exhaustion past relief by repose ; and this happening day after day, and month after month, wore out his body more than the tranquillity and retirement of a few months in the country was sufficient to restore. During the last spring, a manifest change took place in his appearance. Already much wasted, he now became extremely emaciated and feeble; and though the faculties of his mind remained perfect, there were times when these even were deprived of their wonted vigour.

In the early part of the summer, he had an attack of inflammation of the mucous membrane of the trachea, which, though it at first created little disturbance, became, in the month of June, very troublesome, being attended with some fever, and a frequent cough. In this state he quitted London for Tunbridge Wells, and returned in a few weeks, the more teazing symptoms of cough having been relieved by local bleeding and blisters; but in no respect had his general health improved. His feebleness was now so great, that conversation was a considerable effort; and he had lost all relish for food. Though aware that his situation was precarious, he seemed to entertain the hope of being able to return to London in the winter ; for he was persuaded that he had no organic affection, and that by repose, and living in the country, the digestive organs would be to a certain extent restored. Such were the expressions he then used, when adverting to his situation.

With these hopes he went down to his residence in Gloucestershire; but instead of gaining strength after his arrival there, he became daily more and more enfeebled. Besides Dr Barrow, whose attendance was unremitting, he was visited by his friends Sir Henry Halford, Dr Maton, and Dr Warren ; but their efforts, aided by all the tender and anxious cares of an affectionate wife and dutiful children, were unavailing to ward off the fatal blow; and, after much bodily suffering, but with a mind unshaken, he expired on the 23d of September. Thus terminated the life of a man, at once an example to the living for his virtues—his inflexible integrity, his great moral worth, his benevolence to his afflicted fellow-creatures, and for his high intellectual endowments !

Dr Baillie was married, in the thirtieth year of his age, to the eldest daughter of the late celebrated Dr Denman, by whom he had a son and daughter. As has been observed by a contemporary writer, the extent of talent united in his family and connexions is remarkable: not only was he the son of an able Professor, and nephew of the Hunters, but Miss Johanna Baillie, his sister, attained the most elevated rank in literature by her Plays on the Passions. Mrs Baillie's sister was married to the late Sir Richard Croft, a man whose name is endeared in the recollection of many, as well for his manly and upright heart, as for his high professional celebrity; and Mr Denman, the celebrated counsellor, was also Dr Baillie's brother-in-law.

Of Dr Baillie's character in domestic life, it becomes me to say little: a mind so well regulated, and a heart so full of tenderness and benevolence to his suffering fellow-creatures, could not fail to impart joy and affection to the bosom of his family. The pleasure which the constant opportunities his profession afforded him of doing good, the lustre of his career, together with all the blessings of domestic happiness, infused a pleasing glow on his hours of recreation, and inspired everywhere around him a contented cheerfulness.

London, November 28th, 1823.

Report of Diseases of Birmingham, from July 3d to October, 1823.

By John Darwall, M. D.

ACUTE DISEASES.

	No.		No.
Iritis	1	Diarrhœa	3
Prosopalgia	2	Colica	1
Cephalalgia	1	Enteritis	2
Dentitio	2	Hepatitis Acuta	1
Epistaxis	4	Dysuria	1
Cynanche Tonsillaris	8	Nephritis	1
Cynanche Parotidæa	2	Synochus	13
Cynanche Trachealis	1	Typhus	10
Mastodynia	3	Rheumatismus A.	4
Catarrhus	4	Arthritis Rheumatica	2
Pertussis	1	Febris Infantum	7
Pleuritis	1	Roseola	1
Hæmoptysis	2	Variola	1
Obstipatio	16	Podagra	1
Cholera	4	Erysipelas	10

CHRONIC DISEASES.

	No.		No.
Hypochondriasis	1	Paralysis Partialis	3
Cephalæa	22	Epilepsia	3
Mania	2	Staphyloma	1

CHRONIC DISEASES *continued.*

	No.		No.
Oph. Tarsi	1	Amenorrhœa	5
Tussis	10	Menorrhagia	9
Catarrhus C.	1	Syphilis	2
Aphonia	1	Sarcocele	1
Bronchitis Chronica	5	Rheumatismus C.	13
Dyspnœa	1	Lumbago	2
Ph. Pulmonalis	20	Coxalgia	1
Asthma	15	Abscessus	1
Pleurodyne	1	Exostosis	1
Gastrodynia	6	Struma	6
Melæna	1	Lepra Vulgaris	1
Dyspepsia	85	Scabies	9
Asthenia	15	Ecthyma	1
Marasmus	24	Urticaria	1
Tænia	7	Impetigo	1
Dysenteria	3	Ulcus	1
Icterus	2	Anasarca	1
Hepatitis C.	6		
Hydrops	1		403

The present quarter exhibits a considerable decrease in the average number of patients; and indeed, from last April, the town has gradually become more and more healthy. The months of July and August were wet and cold, and great apprehensions were entertained for the ensuing harvest. In September, the weather was fine and open, but without any very hot days. Through the whole of this period, we have been peculiarly free from any prevalent disorder; and it would seem, that the season was too mild to excite those of winter, and too cool to give rise to autumnal complaints. In the corresponding quarter of last year, cholera prevailed much; but in the present list there are only four cases.

Towards the end of July, a few instances of catarrh were observed, some of which were accompanied with sore throat. They were for the most part very slight; and readily yielded to brisk purgatives.

The case of Nephritis was remarkable, as occurring after scarlatina, of which complaint the dropsy, with coagulable urine, is no unfrequent sequel. During the fever, this patient, from the report of her mother, had made very dark-coloured urine, and complained much of pain in her back. These symptoms were not much attended to, it being thought that they would disappear with the original disease. They continued, however, with more or less severity, for three months after the cessation of the fever; and when she came to me, the pain of her back was so intense, that she could scarcely walk or stand upright. The pain was aggravated upon pressing the situation of the left kidney; and there was sickness, with great general disorder of the constitution. Her urine was nearly a dark amber colour, and coagulated strongly by heat. The disease speedily yielded to antiphlogistic treatment, and the urine became natural in its appearance and want of coagulability.

The instance of Aphonia was an excellent example of the efficacy of the tartar emetic ointment, the patient having recovered her voice after repeated blisters had been tried without effect. The eruption was kept out for a fortnight. After, however, it had healed, and the individual apparently quite recovered, the situation of the eruption became very hot and painful, put on the appearance of a common blistered surface when the blister is healing, and discharged a great quantity of fluid so acrid, that, whatever part of the skin it touched was excoriated, and became the source of a similar discharge. It had thus spread over the anterior part of the chest, the upper part of the abdomen, and a little on the back. The patient was much relieved by an application of acetate of lead, with cetaceous ointment; and in the course of three weeks, the skin had again healed.

The case of abscess was the consequence of chronic inflammation between the fascia and the oblique and transverse abdominal muscles. The woman was pregnant, and complained of great soreness in the left iliac region. It was not very tender to slight pressure, but she shrunk much if strong pressure was made. A few days after I first saw her, she died from miscarriage, and permission was obtained to examine the body. The abdomen was tympanitic; and, upon cutting into its cavity, there was a burst of excessively fetid air. There was also at least three pints of a whitish oily fluid in the abdomen, like badly maturated pus. Between the fascia and abdominal muscles of the left side, there was a large cavity, containing also a similar matter, but between which and the abdomen, no communication could be traced. It extended from the pelvis to the diaphragm, and from the linea alba to the spine. It was not lined with a factitious membrane, but the muscle itself was much broken down and softened, and the fluid seemed to have penetrated between the fibres.

Acupuncturation has been tried in an instance of rheumatic pain of the left deltoid muscle, and with complete relief: but this was not so sudden as in many of Mr Churchill's cases. Indeed, three needles were introduced every third or fourth day, for seven or eight times, before the pain completely disappeared.

First Annual Report of the Eye Dispensary of Edinburgh, from the 4th Debember 1822 *to* 18*th November* 1823. Surgeons—JOHN HENRY WISHART Esq., F. R. C. S.; JOHN ARGYLE ROBERTSON Esq., F. R. C. S.

Eight hundred and thirty-nine patients affected with diseases of the eye have applied at the Dispensary for relief, and have been attended either at the Institution, or at their own houses, as the nature of the cases required. Of this number there have been discharged

Cured - - -	604
Relieved - - -	72
Incurable or irregular - -	40
Remain under treatment, -	123
	839

It may however be remarked, that considerable difficulty has been experienced in ascertaining, with certainty, the exact proportion of those who have been cured, relieved, &c. This must always happen in similar institutions, where all the patients are not visited at their own houses; but it is believed that the result above stated is nearly accurate. The Medical Officers have no hesitation in expressing their opinion, that the Eye Dispensary of Edinburgh has been attended with great advantage, not only to the inhabitants of this city, but to those of different parts of Scotland and north of England. But their satisfaction in witnessing the benefits bestowed by the Institution has frequently been damped, by their being called to administer to patients whose diseases had become incurable, merely from neglecting to apply for medical assistance in a sufficiently early stage of the disease.

Seventeen cases of *Iritis* occurred in the course of the year. In all of them the use of mercury was found to be attended with manifest advantage. As soon as the system began to be affected, pain and every untoward symptom gradually abated. Blood-letting and other evacuants were joined to the use of mercury. From an attentive consideration of these cases, and of others which have fallen under our observation, we have been led to conclude, that neither mercury nor syphilis ever produce this disease, and that cold is its most frequent exciting cause. As a detail of these cases, and of the various reasons which have led us to this conclusion, would occupy too much of your present Number, we shall delay the consideration of them till a future period.

Various cases of *Entropion* have occurred, which were successfully treated by the application of sulphuric acid to the external surface of the eyelid. In some cases, a single application was successful; in others, two or three applications were required, allowing an interval of eight or ten days between each.

In cases of *Ectropion*, the nitrate of silver was applied to the conjunctiva of the eyelids, which was attended with great advantage. It corrected the thickened and relaxed state of the membrane; and, by the cicatrizing of the sore, which remained after the slough produced by the application of the caustic was thrown off, the edges of the eyelids were brought into their natural position.

During the changeable weather of spring and autumn, a large proportion of the patients were affected with *catarrhal ophthalmia.* The symptoms being in general mild, they yielded in the course of ten days or a fortnight to the usual remedies. It was observed, that this disease generally attacked all the members of a family in which it appeared.

A considerable number of cases of *Exanthematous ophthalmia* occurred. In such cases, great benefit was derived from the use of mild purgatives continued for some length of time; and from a succession of blisters applied behind the ears, or to the nape of the neck.

In some cases of amaurosis, the application of blisters to the immediate neighbourhood of the eye produced ophthalmia. Judging from this and other circumstances, we are induced to lay down as a general rule, that blisters ought never to be applied near to an inflamed organ; they must always increase the action of the adjacent vessels, and cause an increased determination of blood to the diseased part.

DOMESTIC INTELLIGENCE.

Effects of Iodine in Scrofulous Affections.—Mr Deliſſer, of 55, Judd Street, Brunſwick Square, London, Extraordinary Vaccinator to the National Vaccine Eſtabliſhment, has ſent to us an account of ſeveral caſes, in which he uſed various preparations of iodine with good effect. A young unmarried woman of 19 had been affected, for $2\frac{1}{2}$ years, with a ſcrofulous ſwelling of the knee, and contraction of the joint. After leeching, a liniment of one drachm and a half of iodine, diſſolved and diffuſed through an ounce and a half of rectified ſpirit of wine, was ordered to be rubbed on the knee twice a day, for an hour each time. In ten days, the proportion of iodine was increaſed to two drachms to the ſame quantity of ſpirit. The knee was perfectly reſtored to its healthy ſtate in about a month after the frictions with iodine were commenced; but, to confirm the cure, a plaſter of half an ounce of iodine, with an ounce of ſtrengthening plaſter, was applied. The liniment ſmarted the leech-bites when recent (probably from the alcohol it contained), but afterwards excited no eruption, redneſs, or heat. The only obvious effect was pain in the œſophagus, and if the thyroid, or cricoid cartilages were preſſed, of ſoreness along the throat, and conſtant dryneſs of the glottis; and Mr D. obſerves, that every patient with whom he has uſed iodine, has made the ſame complaints.

The next patient was a child three years of age, born of highly ſcrofulous parents, and itſelf affected in an extreme degree. The iodine was given internally. One drachm was diſſolved in ten drachms of ſpirits; and of this tincture, 15 drops were made into a mixture, with half an ounce of gum arabic, and as much aniſe water, and taken daily in three equal portions. On the 11th day the daily doſe was increaſed to 30 drops, on the 21ſt to 60, on the 45th to 75, and a fortnight afterwards it was diſcontinued. The child recovered completely, having taken 222 grains of iodine in less than two months.

The third caſe was one of a lady with ſcirrhous breaſt, for which amputation was preſcribed. On the 19th of June ſhe began to take a pill, containing one grain of iodine, and two of hemlock powder, three times a day. The doſes of the medicines were gradually increaſed until about the 22d of July, when ſhe began to take 30 grains of iodine, and 18 of hemlock, every 24 hours, which large doſe was continued until August 1ſt. Her appetite then failed; the pulſe, which on the 17th June was 70, roſe to 130; her mouth fell into a ſort of ulceration, with fetid breath, differing, however, from that cauſed by mercury, and, if poſſible, more diſagreeable; but her complaints were little relieved, and the medicines were diſcontinued. On the 9th, the ſymptoms aſcribed to it had entirely diſappeared. Its uſe was reſumed on the 9th, to the extent of 30 grains daily; but on the 20th, all the former ſymptoms returning, and the patient having derived no benefit from it, it was finally abandoned. This lady took in all, between the 19th of June and the 20th of Auguſt, 1019 grains, being the largeſt quantity of iodine ever taken by any individual.

Theſe trials, ſo far as they go, confirm the obſervations of Dr Coindet ſenior, on the ſpecific virtues of iodine in ſcrofula, while they ſhow that it is inadequate to the removal of true ſcirrhus. We have preferred giving this brief account of Mr Deliſſer's communication to withholding the detail for a future Number, that others may be induced to make trial of it in one of our moſt common and intractable diſeaſes. At the ſame time, we muſt caution our readers, that the doſes given by Mr Deliſſer are much larger than thoſe given by Dr Coindet. His tincture was made with *one* part of iodine to *twelve* of ſpirit; and in this form he never gave more than 20 drops three times a day, or ſix grains of iodine (72 to the drachm), daily; and two ounces of the tincture, or 96 grains of iodine were conſidered an ample quantity for a full courſe of the medicine, and ſufficient to remove the moſt obſtinate goître. We ſhould also caution our contributors, that, on account of its inſolubility, the form of pill is the leaſt eligible; and that the iodine itſelf is very inferior to ſome of its combinations, both in efficacy and in convenience.

Trial for Infanticide.—We have often thought, that an occaſional comment on a few of the numberleſs errors and indiſcretions daily committed by medical witneſſes in our courts of law, might conduce in no ſmall degree to the inſtruction of our readers, the intimidation of offenders, and the diffuſion of a taſte for medico-legal ſtudies. But we have been withheld from attempting it, partly by the dread of our motives being miſinterpreted, and partly by the inaccurate reports given in our only ſource of information, the daily papers. The following caſe, however, (for the particulars of which we are indebted to an eſteemed correſpondent), is ſo glaring in its circumſtances, and illuſtrates ſo ſtrongly the danger of that practice, which admits all medical perſons whatever to give evidence, that we have no heſitation in departing, in this inſtance, from our uſual reſerve.

At the laſt Autumn Circuit, a woman was tried at a county town in Scotland for the crimes of child-murder and concealment of pregnancy. She acknowledged the latter crime, admitted that ſhe wanted but two weeks of her full period, and pointed out where the body might be found. A child's body was accordingly diſcovered, as ſhe deſcribed, in the bottom of a preſs, covered with earth, and wrapped in an old petticoat. When it was ſhown to her, ſhe confeſſed that it was the body of her child, and that it was in the ſame ſtate as when ſhe buried it. Two medical gentlemen of reſpectability examined the body. They declared that it was a full-grown child, rather above the ordinary ſize, and dead for ſeven or ten days. (It was examined eleven days after the woman's delivery.) A piece of printed calico had been twiſted and drawn twice round the lower part of the neck, ſo as to produce a very remarkable groove; and another piece of cloth of the ſame deſcription, and doubled, was applied over the head, mouth and chin, and knotted at the back of the neck ſo ſtrongly as to flatten the noſe. The face was turgid and livid; the conjunctiva of the eyes ſtrongly injected with blood. The umbilical cord was divided eight inches from the navel, and had not been tied. No other marks of violence were found on the body. The lungs were perfectly free from putridity, and ſwam in water in the ſame way as when they have been inflated; and the air-cells viſibly contained air. From theſe circumſtances taken together, the two witneſſes deponed, that the child muſt have been born alive, and that its death was cauſed by ſuffocation. Three other medical perſons, who had not ſeen the body, were examined for the priſoner; *and all of theſe gentlemen agreed, that if the child had been dead for the period of eleven days, it was impoſſible for any medical man to come to a concluſion as to whether the child had been alive at the time of birth.* The verdict was returned, Guilty of Concealment of Pregnancy,—Murder not proven.

Such is the ſtatement of the caſe, ſo far as we have been able to acquire information concerning it. It is upon the opinion expreſſed in the lines printed in italics, that we purpoſe to make a few remarks. If that part of the ſtatement be incorrect, the three gentlemen have themſelves to blame for what follows; ſince it was printed in the newſpaper immediately afterwards in theſe very terms; and, if incorrect, it was equally their intereſt and their duty to rectify it. A declaration ſo raſh, ſo hazardous to their profeſſional character, ſo ſubverſive of juſtice, ſo hurtful to the ſtability and reputation of medical evidence in general, it has not been our lot to hear or read in the annals of legal medicine.

We are utterly at a loſs to form even the moſt remote conjecture as to the grounds of this moſt extraordinary aſſertion. Do the gentlemen reſt it on the opinion of authors? We ſhould gladly learn where they can find a ſingle good modern authority (for moderns only are admiſſible) to ſupport them. Will they look to the French medical juriſts? To Mahon, Belloc, Foderé, Chauſſier, Capuron, Lecieux, Orfila? Except Belloc, who is ſilent on the ſubject, each of theſe diſtinguiſhed authors has ſtated the very reverſe in the moſt unequivocal language. Will they look then to Germany? Buttner, Haller, Camper, Daniel, Wildberg, Schmidt, and a hoſt beſides, agree entirely with the French writers. Or will they turn for ſuccour to their own countrymen?—to Smith, Paris and Fonblanque, Hutchinſon, or the Profeſſors of the Edinburgh School? Had they conſulted any of theſe gentlemen, they never would have committed themſelves as they have done. Do they found their opinion, then, on expreſs perſonal experiment? We may ſafely anſwer that it is impoſſible; becauſe experiments have been repeatedly made with the greateſt care by the authors quoted, and have invariably led to the oppoſite concluſion; and had the three witneſſes been led by perſonal reſearches to form an opinion ſo hoſtile to the beſt eſtabliſhed doctrines in medical juriſprudence, it was their bounden duty not to permit its diſſemination, unaccompanied by the experiments on which it reſted. The only reaſon why the fact of the child having been born alive, ſhould not have been recognizable eleven days after death, muſt be the decay of the body. But, even laying aſide the evidence of the crown witneſſes regarding the integrity of the lungs, are theſe gentlemen not aware, that the pulmonary tiſſue is one of the laſt parts of the body to fall into putrefaction? Of the hoſt of authorities we might adduce in proof of this, we ſhall ſubjoin two only, each of whom ſpeaks from perſonal experiment. Camper found that at a period (ſometimes extending to three months) when the whole bones of the body ſeparated from each other under the ſlighteſt touch, the lungs of ſtill-born children *had only begun to decay, and did not ſloat in water.* " If the body of a ſtill-born child," ſays Orfila, in a work publiſhed a few months ago, " be allowed to decay entire, the lungs are obſerved to putrefy with the greateſt difficulty; and they barely preſent ſlight traces of decompoſition, when the ſkin, muſcles, and ſeveral of the viſcera, have paſſed into the ſtate of a liquid, ſtinking maſs; and hence buoyancy of the lungs, in conſequence of putrefaction, muſt be an *exceſſively rare* phenomenon in medico-legal inveſtigations on Infanticide." (*Leçons de Méd. Lég.* I. 269, 1823). But although the lungs had really been putrid, and did float in conſequence of the evolution of gaſes, are theſe gentlemen not aware, that this ſource of fallacy may be obviated—and we preſume it was obviated in the preſent inſtance—by the very ſimpleſt precautions? (See our Number for laſt July, xix. p. 449). And, finally, are they not aware, that, beſides the buoyancy of the lungs, many other collateral proofs of the child having outlived birth may be diſcovered long after putrefaction has begun?

It has been obſerved by many recent writers, that in Britain a conviction for child-murder has become almoſt impoſſible; and that the chief obſtacle is the inſufficient and contradictory evidence of the medical witneſses. The foregoing ſtatement would ſeem to ſhow, as our correſpondent ſuggeſts, that, as long as the moſt unqualified of men are allowed, under the impulſe of miſguided humanity, to volunteer their opinion, no priſoner will ever be convicted, unleſs ſhe has cited perſons to witneſs her criminal act. But we earneſtly entreat our lawyers not to judge of the value of medical evidence by ſuch mortifying exhibitions as that we have been deſcribing. The more we turn our attention to the ſubject, the firmer is our conviction,—and in this conviction we are borne out by every one of the few perſons in this country entitled to the name of Medical Juriſts,—that, to procure a ſatisfactory and irrefragable opinion in caſes of Infanticide, and in all other difficult medico-legal queſtions, it is only requiſite to ſubmit the matter to a diſpaſſionate and ſkilful inveſtigation. Thoſe little acquainted with Medical Juriſprudence, whether profeſſional or unprofeſſional, univerſally confound together *doubt* and *difficulty*. The queſtion involved in the trial which has led to this diſcuſſion, muſt be allowed to be almoſt always difficult; but we are certain that, when properly examined, ſcarcely one inſtance in a hundred will prove doubtful.

In alluſion to the preſent caſe, we have only farther to add, that, although the crown witneſses appear to us to be in the right, ſo far as can be judged from the ſhort report put into our hands; yet we are pretty ſure that, from a minuter inquiry, they would not eſcape altogether free from blame. This is ſhown by the anſwer returned to a queſtion put by the priſoner's counſel, as to the poſſibility of the child having died of hemorrhage from the untied umbilical cord. They acknowledge it was poſſible. Now, that point ſhould have been poſitively determined by inſpection. If the cord was divided by a cutting inſtrument, the hemorrhage might have been fatal; if it was torn acroſs, hemorrhage was exceedingly improbable, or, as ſome think, impoſſible. But if the child did die of hemorrhage, the ſigns of *anæmia* or exſanguification, if looked for, could not have been miſſed, or miſtaken.

FOREIGN INTELLIGENCE.

Existence of Iodine in the Mineral Waters of Sales. (Annales de Chimie et de Physique, Aout 1823. xxiii. 419.—The waters of Sales, in the province of Voghera in Piedmont, rise from an argilo-calcareous soil, and collect in a basin 6 feet in diameter, and 19 or 21 inches deep. Their temperature is that of the air; their specific gravity 1.0502. They are muddy, and have a slight yellow tint. The smell is strong, resembling that of urine acted on by muriatic acid, their taste sharp and brackish. Bubbles of elastic fluid are constantly escaping from the bottom of the basin. In 1788, Volta discovered muriate of soda in this water; and in 1820, an Italian of the name of Romana, likewise detected several earthy muriates, and a little oxide of iron. Very recently, an apothecary at Voghera, S. Angelini, discovered the existence of iodine in it, by casually observing that Fecula struck a blue colour with it. He even succeeded in extracting from it a notable quantity of iodine, by the process used for preparing that substance from saltpetre-makers' ley. "It is a singular fact, that for a long time the waters of Sales have been successfully used in the treatment of scrofulous disorders, and especially of goitre."

Vegetable Milk. (Annales de Chimie et de Physique, Juin 1823. xxiii. 219.) A few years ago, Humboldt published in his Voyage to the Equinoctial Regions, a curious account of the Milk-tree, or Cow-tree *(Arbol de leche. Palo de Vaca)*, a native of Venezuela, belonging to the natural family of the Sapotæ,—which is said to yield a milky juice, highly nutritious, and possessing almost all the properties of cow's milk. That distinguished traveller visited the district where it grows, and found its qualities had not been at all exaggerated. He observes it has all the external characters of cow's milk, except that it is somewhat viscous, and has a pleasant balsamic odour. He drank it plentifully in the evening, and early in the morning, without experiencing any unpleasant effects; and he was informed by the overseer of a farm, that when it is in season, the working people use it with their cassava bread, and are always observed to fatten in consequence. Pellicles form on its surface, when it is exposed to the air, and more especially when it is heated. These pellicles are called cheese by the inhabitants; and, on a cursory examination, were supposed by Humboldt to be composed chiefly of real *caseum* (the peculiar proximate principle of true animal cheese), and of caoutchouc and albumen. The juice, and its several component parts, putrefy just like milk. (*See the same Journal*, vii. 182.) Very recently this juice has been chemically examined with great care by MM. Boussingault and Mariano de Rivero; and its solid ingredients have proved to be chiefly wax, a little sugar, and a considerable quantity of a matter possessing all the physical and chemical qualities of animal fibrin. The juice is miscible with water, does not coagulate with the acids, and scarcely with alcohol, and furnishes no precipitate with ammonia. The last test shows it contains no caoutchouc. It reddens litmus; but to which acid this property is owing was not determined. By evaporation it is reduced to an extract resembling *frangipane*. By then increasing the heat, it separates into an oleaginous fluid, and a fibrous mass floating in the fluid; and when urged by heat still farther, it gives out the exact smell of flesh in the act of being fried. The oleaginous matter concretes on cooling into a translucent, faintly yellow mass, sufficiently hard to resist the impression of the finger, completely fusible at 140° F.; soluble in the volatile oils; combining with the fixed oils so as to form a cerate; soluble in hot alcohol, but precipitating when it cools, and forming a soap with the fixed alkalis;—in short, exhibiting all the properties of purified bees'-wax. The fibrous matter, when thoroughly deprived of the wax by the action of some essential oil, is brownish and tasteless; is acted on by heat just like a bit of muscular fibre; and, when heated with nitrous acid, emits an elastic fluid, which appears to be nitrogen gas. It is insoluble in alcohol; and, when repeatedly washed with it, exactly resembles animal fibrin in external appearance, as well as in the chemical relations just noticed. This is the second plant in which fibrin has been detected. In the year 1802, Vauquelin published an analysis of the juice of the Carica papaya, or papayer; and there stated, that he had found the residue of part of his process to exhibit under the action of heat all the phenomena of a bit of meat roasting, and to resemble exactly the fibrin of the blood. (*Ann. de Chim.* xliii. 269.)

Analysis of Maize. (*Annali Universali di Medicina. Dicemb.* 1822. xxiv.) The following analysis of Virginian maize was executed by Dr Gorham of Cambridge in the United States.

Water	9.0
Fecule	77.0
Zeina	3.0
Albumen	2.5
Gummy matter	1.75
Saccharine matter	1.45
Extractive	0.8
Epidermis and lignin	3.0
Phosph. carb. and sulph. lime	1.5
	100.

The substance which the analyst has denominated Zeina, is most easily procured by digesting a few ounces of maize-flour for some hours in warm alcohol contained in a phial, and then filtering the solution immediately, and distilling off the alcohol. When so prepared it is yellow, and resembles wax in appearance; it is soft, ductile, tenacious, elastic, without taste or smell, and heavier than water. It smells, and burns when heated, emitting the odour of animal matter in combustion; but yields no ammonia. It is insoluble in water or the fixed oils, but soluble in alcohol, in oil of turpentine, in sulphuric ether, and partly also in the principal acids and alkalis. It is not decomposed by the action of the atmosphere; for the discoverer kept some of it six weeks unaltered. It resembles gluten more than any other vegetable principle, but differs from it by its solubility in alcohol and ether, and by containing no azote.

On the Direct Passage of Substances into the Blood. (*Journal Complementaire, &c., Septembre,* 1823. xvi. 235.)—Although the frequently repeated experiments of Flandrin, Magendie, Mayer, Gmelin and Tiedemann, leave little doubt as to the direct passage of many substances into the venous system, it gives us additional pleasure to notice a farther confirmation of them by a skilful physiologist, M. Westrumb of Hamelin. His experiments are the following. The sulphuretted hydrocyanate of potass, indigo, rhubarb, oil of turpentine, were injected into the stomachs of dogs and sheep; and after various intervals of time, the animals were killed and carefully examined. When a mixture of indigo and oil of turpentine had been used, and the animal was killed 5 hours after the beginning of the experiment, the two substances were plainly manifested to the sight and smell in the whole tract of the intestines, as well as in the blood of the vena portæ, in the substance of the lungs, liver and kidney, and in the urine; but not a vestige could be recognised in the glands of the mesentery, or in the chyle. The oil of turpentine and the hydrocyanate of potass were given to a sheep in repeated doses during four days, and it was killed half an hour after the last dose. Neither of these easily detected substances could be found in the lymphatic vessels or glands of the mesentery, or in the thoracic duct; but both of them were very obvious in the liver, kidney, spleen, and blood of the vena portæ. Similar results were obtained with the decoction of rhubarb, and the sulphuretted hydrocyanate of potass. Tying the thoracic duct near its termination did not alter the phenomena. The researches of Westrumb likewise confirm those of former experimenters as to the extreme rapidity of the venous absorption. He detected rhubarb in his own urine five minutes after swallowing an ounce of its infusion; and after the same period in rabbits, but not earlier. Half a grain of the hydrocyanate of potass given to a little dog, became sensible in the urine *at the end of two minutes.*

On the Influence of the Nervous System over the Secretion of Urine. (*Ibidem*, p. 225.)—The same physiologist has supplied some curious facts on the share which the nervous system possesses in the secretion of urine; and he infers from them, that without the assistance of the nervous fluid, this secretion does not take place at all. After severing the spinal column from the brain, betwixt the skull and first vertebra in a rabbit, he injected into the stomach a dram of hydrocyanate of potass dissolved in water; and proceeded to maintain the action of the head by artificial respiration. The temperature which, from being 108½ Fah. before the division of the spine, had fallen to 97¼, rose gradually in 15 minutes to 103, and then gradually fell to 82½. The insufflation was then discontinued, viz. an hour after it was begun, and the organs were carefully examined. The trito-sulphate of iron detected the presence of the hydrocyanate in the serum of the aortal blood, and that of the vena portæ, in the respiratory mucous membrane, in the substance of the liver, spleen, lungs, *and in that of the kidneys; but not a trace was perceptible in the urine, of which the bladder contained only a few clear limpid drops.* This experiment was repeated upon a dog; but two ounces of the hydrocyanate were given. The substance of the kidneys acquired a beautiful greenish-blue colour with the ferruginous test; the bladder, which the experimenter had forgot to empty at the beginning of the experiment, contained a large quantity of urine without a vestige of the hydrocyanate; at the same time that salt existed abundantly in the blood-vessels of the pelvis.

These are certainly powerful facts, for no substance passes in ordinary circumstances so rapidly into the urine; we have already seen, that it may be detected there two minutes after being swallowed. They are also confirmed by the results of a set of experiments even more interesting, performed in 1820, by a German physiologist M. Krimer. This gentleman observed a total suppression of urine after the division of the spinal cord; and when only the renal nerves were divided, he found that the urine yielded much less urea, uric acid, phosphates or hydrochlorates, and no rhubarb (which had been previously swallowed), but a much larger quantity of albumen. It is also a singularly interesting fact (which must recal to our readers the ingenious researches of our countryman Wilson Philip), that by means of a current of galvanism, the passage of substances into the urine, which had been interrupted by the division of the parvagum, is completely re-established. M. Krimer divided both of these nerves in the dog, emptied the bladder, and injected a decoction of rhubarb into the stomach. The urine repeatedly examined with caustic potass, gave no indication of the presence of rhubarb. Three days afterwards, he gave the animal a fresh quantity of rhubarb, and connected the lower ends of the two divided nerves with a small voltaïc pile; in a short time the presence of rhubarb in the urine was distinctly detected; and on interrupting the galvanic current, it again speedily disappeared. These experiments of Krimer are contained in a work entitled *Physiologische Untersuchungen*, or Physiological Researches, published at Leipzick in 1820.

Inflammation of the Tongue Cured by Incisions. (*Journal Universel, &c.*, *Juin* 1823. xxx. 367.)—A stout young woman, after exposure to cold, experienced a considerable diminution of the menstrual discharge; and not long afterwards was attacked with severe pain of the throat, impeding deglutition and the movements of the tongue. The tongue then began to swell, and soon became so voluminous, as to block up the fauces, project out of the mouth, press down the lower jaw, and cause a distressing sense of approaching suffocation. At the commencement of the disease, irritating clysters were given, leeches were applied to the vulva, and blood drawn from the feet, but in vain. At last her attendant had recourse to the expedient of making two deep incisions from the base to the tip of the tongue. A few hours afterwards, the swelling had abated so considerably, that the woman was able to close the jaw. The tartrate of potass and antimony was then prescribed, even so as to produce vomiting; her state continued rapidly to improve, and she was completely well on the eighth day. This severe practice has been resorted to by many practitioners in similar circumstances, and almost invariably with success. The author of the paper from which we have extracted the foregoing notice, refers the reader in particular, to a treatise on the subject, in the 5th volume of the *Mémoires de l'Académie de Chirurgie*, by de Lamalle, who professes to have derived it from Job-a-Meekren, a well-known Dutch surgeon of the 17th century.

On the Use of the Sulphate of Kina in Intermittent Fever. (*Giornale Arcadico di Scienze, &c. Novembre,* 1822. xlvii. 129.)—The following statement by de Rossi of Anani and Tonelli, a physician at Paliano, is derived from the most precise and extensive experiments yet published on this subject. Both of them have made up a set of admirable tables, from which we have deduced the subsequent particulars. De Rossi used the sulphate of kina in 64 cases, namely in 8 simple tertians, 29 double tertians, 2 simple quartans, 17 subcontinued, and 8 pernicious intermittents. *All of these were cured; 50 had no subsequent paroxysm, 7 had only one very slight fit, 5 two similar fits, 1 several fits, and 1 passed into a continued remittent fever.* One person affected with a quartan for 12 months, recovered after a single slight return of the paroxysm. The quantity of the salt used varied between 12 and 72 grains; and in 45 cases did not exceed 24. The remedy was given in pills, and always in the apyretic intervals, in doses of 4 grains every hour, or every alternate hour; and, in general, to confirm the cure, 4 grains were given daily for 10 days after the last paroxysm. The principal advantages to be derived from it, in preference to the bark itself, are the following:—It may easily be given to children, and to those who have a disgust at bark, or are affected by it idiosyncratically; and it has a rapidity of effect, especially in pernicious and sub-continued intermittents, far surpassing that of the bark. He farther proves, that large doses produce no bad consequences; in other words, that it can never act poisonously; that it may be given safely to persons of all ages and temperaments, to puerperal, pregnant and menstruating females; and that, when it fails, the failure is owing either to the existence of some other morbid cause, or to the sulphuric acid being present in excess, or to the salt being decomposed by pharmaceutic substances intermingled, or by other matters which it encounters in the alimentary canal. The observations of Tonelli are equally pointed. He used it in 65 cases, namely in 4 quotidians, 22 simple tertians, 31 double tertians, 3 simple quartans, 2 double quartans, 2 subcontinued, and 1 pernicious intermittent. All of them recovered, except the last, who would not take the remedy till he was insensible, and near the point of death. Forty-two recovered without any return of the fit; of the 65 cases only 43 required a repetition of the first dose, which varied from 12 to 18 grains. These results, especially those of De Rossi, who used the salt in greater quantity than Tonelli, and in a greater number of bad intractable cases, place the superiority of the sulphate of Kina beyond all doubt, both as to convenience and efficacy.

Application of Galvanism to the Extraction of Urinary Calculi. (*Annales de Chimie et de Physique, Juin* 1823. xxiii. 202.)—M.M. Prevost and Dumas, the Genevese physiologists, whose ingenious researches on the blood and on urea, we noticed in our last Number, have just published the outline of some experiments they have made, to prove, that the extraction of urinary calculi from the bladder may be facilitated by means of galvanism. They found that when a fusible calculus (ammoniaco-magnesian phosphate) was placed between the extremities of the conductors of a trough containing 120 plates, and was plunged in this state under water, the bases and the acid were gradually transported to the opposite conductors, and there combined anew in the form of a fine powdery precipitate. The process having been continued upwards of 16 hours, a calculus which weighed originally 92 grains became so friable, that the slightest touch broke it down into small crystalline grains, none of them larger than a lentil. They next proved to their satisfaction, that a pair of conductors, protected by some non-conducting substance to within a few lines from their extremities, might be introduced into the bladder of a dog, and connected with a trough of one hundred and thirty-five plates, without occasioning any uneasiness to the animal; although the same apparatus immersed in water decomposed it so rapidly as to furnish a *torrent* of gases. Having thus found that no harm could result from disengaging the galvanic fluid in the bladder, they finally proceeded to introduce the same conductors into the bladder of a middle-sized bitch, but with a fucible calculus fixed between them: the bladder was then distended with water, and the conductors were connected with the galvanic trough. After the calculus had been thus acted on, for an hour twice daily, during six days, it had lost more than an eighth-part of its weight, and had become so weak and brittle, that it could not be introduced again into the bladder. The animal was killed a few days afterwards, and the bladder was found to be quite free from injury or disease.

These experiments certainly prove that a small fusible calculus, when brought between the conductors of a galvanic battery, undergoes gradual decomposition within the bladder. The authors infer, that all other calculi consisting of compound salts, may be decomposed in the same manner; and they anticipate with considerable confidence the application of their discoveries to the treatment of urinary calculi in man. We have no wish to damp their ardour in these curious investigations. But any one may recognise, that in the list they have annexed of difficulties which remain to be encountered, and which they propose to make inquiries upon, they do not include some of the most obvious and insurmountable. Farther, as they themselves remark, the projected plan of treatment cannot be applied to calculi of uric acid, which, according to the statements of Mr Brande, forms the whole or greater part of three-sevenths of the stones analyzed by him.

Application of the Stethoscope to the Diagnosis of Fractures. (Arch. Gén. de Méd. Aout 1823, ii. 631.)—M. Lisfranc, the well known lecturer on operative surgery at Paris, has completely succeeded in realizing a conjecture started some time ago by Laennec, with regard to the applicability of his stethoscope to the diagnosis of fractures. With the aid of this instrument, he avers that no surgeon can ever remain in doubt as to the existence of a fracture in any part of the body, except the head. The tumefaction around the injury can never be so considerable as to obscure the crepitation entirely; and the slightest possible movements are sufficient to produce the requisite sound. The following general rules have been established by frequent observation, both on the dead and on the living body. 1. The instrument may be applied over the seat of fracture with or without the end-piece; but the farther it is removed from the seat of injury, the sound is the more distinct when the end-piece is removed. 2. When the bone is near the surface, the sound is always most distinct; the slightest motion elicits it; and it is most sensible just over the fracture. Hence we may thus discover the precise seat of the injury. 3. The crepitation diminishes as the instrument recedes from the fracture, yet may be felt at a very great distance. 4. When the bone rides, the sound is less obvious; but it becomes more distinct after extension and counter-extension. 5. The crepitation of compact bones gives a sharp, strong crackling, even sometimes so loud as to grate upon the ear. 6. That of spongy bones resembles the action of a file on such a substance as pumice, interrupted, however, by occasional sharp crackling. 7. Oblique fractures crepitate more distinctly than those that are transverse. 8. If liquids are effused round the broken ends, a sound is superadded, like that of the foot when thrust into an old shoe soaked with water. 9. When the fracture is complicated with splinters, the crepitation is united with a crackling as of several cornered bodies rubbing each other. 10. When it is complicated with laceration of the soft parts, the crepitation is united with a sound like that of a person breathing forcibly with the mouth wide open. 11. Dislocations give a very obscure dead sound, almost confined to the seat of the displacement. 12. The sliding of the tendons in their sheaths causes a full, dead sort of shock, quite different from crepitation.

The ingenious author maintains that two advantages attend this application of the stethoscope. In the first place, some fractures will be at once detected, which can be recognised in no other way; and, secondly, fractures of every sort may be easily and accurately recognised, with far less pain to the patient. Although we have not yet made trial of the stethoscope for this purpose, yet from what we know of its utility in pectoral diseases, and of the common principles of acoustics, no doubt can be entertained of the correctness of M. Lisfranc's experiments. It is only necessary to warn the reader, that, in employing the instrument of Laennec, either for this or any other purpose, he must not expect to succeed at the first attempt. To use it successfully requires considerable previous study, and perhaps also some tuition,—a fact, which never seems to occur to the numerous practitioners in our country, who neglect and despise it, without having taken any pains to appreciate its merits. M. Lisfranc has also applied it to the detection of urinary calculi, of biliary calculi, and of hydarthrosis. The first of these applications is useless; the fingers which hold the sound being just as fit for the purpose as the stethoscope held over the pubis. Out of many trials, the author detected the presence of biliary calculi in one instance only. Its use in hydarthrosis is obvious.

Detection of Poisoning with Arsenic a month after Interment. (Archives Générales *de Médecine, Aout*, 1825, ii. 58.)—It is not long since we had occasion to notice an instance, from the same excellent Journal, of the successful application of chemical analysis to the discovery of poisoning by arsenic. The following is a still more admirable example of the necessity of patient and complete examination in medico-legal inquiries. Professor Orfila, and three other medical gentlemen, were requested by the *Procureur du Roi* to examine the body of a person who had been dead about a month, and was buried in the cemetery of Père-Lachaise. When the body was disinterred it emitted a very disgusting smell, which was removed, however, by ablution with the disselved chloruret of lime.

The skin was everywhere of a mottled green or brownish colour; the epidermis and nails could be peeled off with facility; the subcutaneous cellular tissue was blown up with gases, and at some points the skin had begun to break down into *putrilage*. Notwithstanding these signs of advanced putrefaction, the inspectors proceeded to explore every cavity in the usual manner. Few appearances of note were detected in the head, thorax, or pharynx. A few ounces of an oleaginous fluid (changed by putrefaction) were found in the sac of the peritoneum. At the splenic end of the stomach, before the vasa brevia, there was a yellow spot externally, corresponding to a similar spot on its inner surface. Various reddish stains were scattered over the villous coat, and at the pyloric end some ecchymosed spots were apparent; but no decided evidence existed of these appearances having been the result of inflammation. The stomach, duodenum, and jejunum, were lined internally with a yellowish mucus; the ileum was empty; no part of these intestines exhibited any trace of inflammation. "Nothing particular had yet caught our attention," continues the narrator, "when at last we encountered a little whitish particle, which, when examined with a magnifying glass, seemed to possess all the physical properties of the oxide of arsenic. A few more grains of the same description were then detected in the parts which we had already examined. These grains exhaled an alliaceous vapour when projected on burning charcoal; and their solution in water gave a yellow precipitate when treated with hydro-sulphuric acid with the aid of heat. The large intestines were then opened, and there we procured a great abundance of the same white powder; which was submitted next day to all the necessary tests, and distinctly proved to be the white oxide of arsenic." The relator adds, that the greatest quantity of powder was always found where the yellowish mucus most abounded.

The circumstances under which the crime was effected being rather singular, we shall extract them as they are related in the London Courier for the 2d of last December.—Madame Boursier, the wife of the deceased, had, for some time, indulged in an illicit intercourse with a Greek of the name of Kostolo. On the 28th of June, the husband complained to the girl who used to prepare his breakfast, that his rice had a very disagreeable taste; yet, only a few minutes before, the girl, according to her daily custom, had retained a portion of the same dish for herself and one of the children, and both had eat it without perceiving any bad taste, or experiencing any inconvenience. During the interval betwixt the preparation of it in the kitchen and the time when M. Boursier partook of it in the parlour, his wife had been repeatedly passing and repassing from one room to the other. Not long afterwards the deceased was attacked with vomiting, which recurred frequently and violently till his death, which happened forty-three hours after the meal supposed to have been poisoned. The medical gentlemen asked permission to examine the body; but this was refused; and it was hurriedly interred next morning. Notwithstanding these suspicious circumstances, and the positive discovery of poison in the body, the woman was declared not guilty; but the report does not inform us what presumptions were in her favour.

New Periodical Work on Natural History.—We are requested to intimate, that at the commencement of the present year, a Periodical Work will be commenced in Paris, by Messrs A. Brongniart, Audouin and Dumas, exclusively appropriated to Natural History. It is to be entitled, Annales des Sciences Naturelles, and will be published monthly, in the form of an octavo pamphlet of seven sheets, with a quarto Atlas of Plates. From the well known talents of the Editors, and the abundant sources of information which are open to them, the work is expected to be highly interesting and valuable; and will be commenced with the fairest prospects of success.

Notice by Dr Abercrombie of M. Louis's Cases of Perforation of the Intestines in Acute Diseases, referred to in page 9th of this Number.

I.—A man, aged 24.—Had feverish symptoms, with pain of his back and loins; was better after a few days, and returned to his usual employments; but, after some days, relapsed; had then headach, pains of his limbs, white loaded tongue, thirst, hot skin, and slight pain of the belly, when it was pressed; bowels rather confined; pulse frequent; after leeches, glysters, and fomentations, was better; pain of belly gone; but the symptoms returned, from improper diet, and were again relieved by the same means; and about the 12th day of the disease he was entirely free from uneasiness, though some heat and fever continued. On the three following days he was considered as convalescent; the pulse down, and no complaint. In the night of the 3d day, was seized with vomiting and severe colic pains; belly distended and tender; pulse small and frequent. Died in 20 hours. Dissection.—Bowels adhering extensively; feculent and bilious matter in the cavity of the abdomen, filling the lower part of it. At the lower end of the ileum, near the caput coli, there was a perforation of the intestine two lines in diameter. It was connected internally with an ulcer two and a half inches in circumference, circular, and well defined, with red and thickened edges. Near it, extending along the ileum, there were eight or ten ulcers, of various extent, but not perforating. They appeared not to extend beyond the mucous membrane; there was no inflammation around them, and their edges seemed disposed to cicatrize.

II.—A man, aged 22.—20th March, 1822.—Had headach and flushing suffusion of the eyes; white tongue, thirst, quick pulse, bowels open, and no complaint of the belly. 21st. Slight pain of the belly, which was relieved by leeches and a glyster. All the symptoms then subsided; the pulse came down gradually; and, on the morning of the 28th, he was considered as convalescent. Having then exceeded in animal food, he was seized with rigors, followed by fever and some delirium; pain in the region of the cæcum; stools frequent, liquid, and brown; some relief from leeches; but the symptoms returned in the night with great severity; and he died in the morning of the 29th. Dissection.—Brown fetid matter in the cavity of the abdomen, mixed with flocculi; small intestines red. In the ileum, six inches from the cæcum, there was a perforation the size of a writing-quill, connected internally with an ulcer the size of a 30 sous piece. The mucous membrane around it was inflamed, and presented, in several places, red projections well defined.

III.—A young man of 17.—13th October, 1822.—Was seized with oppressive and general uneasiness, heat of skin, and loss of appetite. 16th. Continued in the same state, with foul tongue and thirst. Bowels rather slow; no complaint of belly; continued without any other symptom to the 22d; bowels moved once in the day; tongue cleaning; pulse continued frequent, but moderate. 23d. Some increase of fever, with pain of the abdomen increased by pressure; two liquid stools. In the night, the pain became extremely violent, with much bilious vomiting. 24th. Great pain and tenderness of abdomen; small frequent pulse; quick breathing; nausea and occasional vomiting; no stool. 25th. Sinking, with less suffering; belly tumid and tense, but less tender; vomiting continued; no stool. Died in the afternoon. Dissection.—The abdomen contained a turbid puriform fluid, mixed with flocculi; extensive adhesions of the intestines. In the ileum, ten inches from the cæcum, there was an opening perforating the intestine, and communicating with an ulcer of the mucous membrane, five lines in diameter. Betwixt the ulcer and the cæcum there were numerous pustular elevations covered by the mucous membrane, and three or four irregular elevations of a grey colour, an inch or more in diameter.

IV.—A woman, aged 26.—Feverish, with headach; spasms of the throat, and a convulsive motion of the lower jaw; quick breathing, with rattle in the throat; some cough and bloody expectoration; frequent pulse, and some delirium. In the night of the 10th day of the disease, was attacked with violent pain of the belly, which continued on the 11th day, with distention and great tenderness; quick feeble pulse; paleness; tongue dry, and partly black; bowels slow. In the night, vomiting began. 12th day. Sinking, with continued nausea; numerous loose stools; epigastrium extremely tender; pulse very small. Died next morning, having passed the night with great distress and constant vomiting. Duration of the peritonitis, from 50 to 54 hours. Dissection.—Soft and recent adhesions of the intestines; some inflammation of the omentum, with adhesion to the intestines; much turbid puriform fluid in the abdomen, mixed with flocculi. In the ileum, 3 inches from the cæcum, there was a perforation two lines in diameter; and, some inches from it, two smaller openings. Internally, they all communicated with ulcers, and besides these, there were several other ulcers not penetrating, amounting to about ten, in a space of 18 inches of the intestine. Their margins were generally irregular, and somewhat elevated, and the bottom of them was formed by the muscular coat. The mucous membrane of the part was thickened and soft. The mesenteric glands corresponding to the diseased portion were much enlarged and red.

V.—A man, aged 25.—Irregular attacks of chills and heat; impaired appetite; uneasiness after eating; costiveness; but no pain of the belly, and no frequency of pulse. After being in this state rather more than three weeks, he was suddenly seized in the night with violent pain of the abdomen, much increased by pressure, and by the least motion; quick small pulse; and, after some hours, vomiting. During the day, the vomiting subsided; the pain continued; pulse 145, and small. Died on the following day, about 36 hours from the attack. Dissection.—Adhesion of the intestines, and fetid fluid in the abdomen, as in the other cases. In the ileum, 12 inches from the cæcum, there was a perforation 2 lines in diameter, communicating internally with an ulcer. Along the lower half of the small intestine, there were numerous elevated portions of the mucous membrane, from 6 to 13 lines in diameter, of a grey colour, variegated with blue, from one-half to three-fourths of a line in elevation. At the lower part, many of these spaces presented various degrees of ulceration; and, still lower, were found deep penetrating ulcers. Where the ulceration was small, the mucous membrane around it had the character of these elevated portions. Where the ulceration was more extensive, the surroundtng membrane had a kind of grey puffiness. In one of these ulcers was the perforation.

VI.—A man, aged 30.—Had enteritic symptoms, with fever, but not severe. Bowels moderately open. After three days, was seized with violent pain in the hypogastric region, with vomiting, and died in 36 hours. Dissection.—Extensive adhesion of the intestines; mesenteric glands enlarged and red; a perforation of the ileum five inches from the cæcum, communicating internally with an ulcer. The lower two-fifths of the small intestine presented numerous diseased portions of the mucous membrane, of two kinds. Those of the one kind were grey, variegated with blue, formed by thickening of the membrane; some of them with slight ulceration. Those of the other kind were more prominent, from a line to a line and a half in elevation, with or without ulceration. Those at the lower part gradually degenerated into deep defined ulcers, and the mucous membrane around them was generally softened. There was also an ulcer of the inner surface of the stomach, near the pylorus.

VII.—A man, aged 23.—Had suffered repeated attacks of diarrhœa, with fever, but not severe; had been ill with one of these attacks for fifteen days; stools numerous, sometimes twenty in a day; thirst; heat; anorexia; and some fever; but no pain of the belly. After another day or two, the fever increased, with some delirium in the night; shiverings; stools less numerous. Next day, constant delirium, with shivering; belly distended and slightly tender. Died the same day. Dissection.—Intestines distended and adhering extensively. Six inches from the cæcum, there was a perforation of the ileum, communicating with an ulcer; near it there were three or four other ulcers not penetrating; and along the whole course of the small intestine there were numerous diseased portions of the mucous membrane like those described in the former case.

[*Note by the Editor.*—The following Case, communicated to us when the last sheet was going to press, appears so interesting in various points of view, that we have preferred inserting it here, to delaying its publication for three months.]

APPENDIX TO THE ORIGINAL COMMUNICATIONS.

Case of Ligature of the Carotid Artery, on Account of Hemorrhage from the Inferior Maxillary Artery. By E. W. Duffin, M. D. Fellow of the Royal College of Surgeons, Edinburgh.

On the 11th instant, early in the forenoon, I was requested to visit Lilias French, a delicate child nearly four years of age, who was reported to have been bleeding profusely from the mouth, from about one o'clock in the morning, in consequence, it was supposed, of the rupture of a vessel, occasioned by an attack of vomiting that had supervened during the night.

I found her lying on her left side, insensible, and completely blanched from loss of blood. The breathing was anxious and hurried, the extremities were cold, the pulse extremely quick, when felt in any of the larger arteries, but imperceptible at the wrist or ankle. An extremely fetid odour, like that of a sloughing part, was exhaled from the fauces; the mouth was half open, and filled with a coagulum of blood which protruded between the teeth, and florid blood, though in no great quantity, trickled over the left and depending angle of the mouth. A soft doughy, colourless tumour occupied the whole extent of the right side of the inferior maxilla, extended along the base of this bone, and under the tongue. Upon seperating the teeth to examine the nature of this tumour, a portion of clotted blood, situated over the molar teeth, suddenly gave way, and instantly a quantity of arterial blood gushed from the part, appearing to come from between the gums and alveolar process. The coagulum was immediately replaced, and pressure applied to the bleeding point by means of the finger of a bystander, until some more permanent method of restraining the hemorrhage could be had recourse to, which, on its first attack, had apparently been prevented from proving fatal only by the supervention of syncope.

A basin, containing about an English quart (*i. e.* a choppin) of blood was presented to me, which, I was informed, had been collected upon the accession of the bleeding; and I was assured, that more than half as much again had been lost before I saw the patient.

Conceiving, from the present exhausted state of the child, and the ungovernable nature of the hemorrhage, that the only means of affording even a chance of recovery consisted in tying the main trunk of the bleeding vessels, the necessary instruments were immediately procured, and, returning with my friend Mr Syme, to whose kind assistance I owe many acknowledgments, I immediately proceeded with his aid, and that of his pupil Mr Evans, to pass a ligature round the lower third of the common carotid artery, in the usual mode of performing that operation; and, from this period, the hemorrhage ceased.

In about twenty minutes afterwards, the pulse returned in the wrist, the extremities acquired a considerable degree of heat, and the breathing improved.

As the child was unable to swallow, a glyster of arrow-root mucilage, with a little spirits, as the readiest stimulant that could be procured, was administered, and with apparently good effect, as the sensibility greatly increased, and the powers of deglutition returned in rather better than an hour. Beef-tea and wine were then given in small quantities at intervals, by means of an injection-bag, and every symptom augured favourably until about eight o'clock *p. m.*, when the pulse rapidly sunk again, slight convulsions attacked the face, and the girl died.

Upon inquiry, it appeared that this patient had suffered excruciating agony in the situation of the disease for a period of more than three weeks; but as this was considered to arise from toothach or gumboil, no advice was taken.

DISSECTION.

An incision being made through the integuments from the angle of the jaw to that of the mouth, and another bisecting this, which was continued in a straight line along the side of the neck to the point at which the artery was tied; the skin and cellular texture were dissected back, so as to expose the tumour in its whole length, which appeared of a purple colour, extended from the ramus of the inferior maxilla to the symphysis menti, and passed under the tongue, external to the glossal muscles, but internal to the digastric, including the jawbone in its centre.

The inferior maxillary gland was much diseased, softened in texture, and of a brown hue. Upon cutting into the tumour, it was found to contain about three ounces of grumous blood, the sac being formed by the periosteum, which was separated from the bone in its whole length, both internally and externally, from the capsular ligament of the articulation to the symphysis of the chin, and at the base of the bone, to a little beyond this point. Upon sawing through the bone a little to the left of the symphysis, and removing the right half of the jaw, it was found that the inferior maxillary artery was of very considerable size, diseased in its whole extent, of a dirty brown colour, easily lacerable, and so loosely attached to its osseous canal, that it was withdrawn from it entire, in the same way as a clot of blood may frequently be taken from a vein after death. It appeared ulcerated immediately at the point where it gave off the first branch to the teeth, and there existed at the corresponding portion of the bone a carious aperture, through which the blood had issued and insinuated itself as described. The last molaris was carious, and the other teeth of the same side were loose in their sockets. The body of jaw, as might be expected, was discoloured and dead, and the gums were in a sphacelated condition.

In all probability this case might have had a more favourable issue, could the nature of the disease have been ascertained at an earlier period; but unfortunately it was incurable before advice was sought for.

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PART I.

ORIGINAL COMMUNICATIONS.

I.

CONTRIBUTIONS TO THE PATHOLOGY OF THE STOMACH, THE PANCREAS, AND THE SPLEEN.

By JOHN ABERCROMBIE, M.D., FELLOW OF THE ROYAL COLLEGE OF PHYSICIANS, EDINBURGH.

PART II.

DISEASES OF THE PYLORUS.

THOUGH the affections of the pylorus seldom admit of medical treatment, there are several points connected with the pathology of them that are worthy of attention. The disease usually begins with obscure symptoms, like a slight dyspeptic affection—obscure pain, indigestion, uneasiness after eating, &c.; and these symptoms go on increasing gradually, until, after a considerable time, the case puts on those appearances which indicate the existence of fixed and organic disease. In other cases, the first symptoms are more severe, having the characters of an acute attack; and after the first violence of them has subsided, periodical vomiting continues and becomes permanent.

When the disease has reached a certain period of its progress, we usually expect to find it characterized, by periodical vomiting, returning with great regularity at a certain period after a meal; with fixed uneasiness at the epigastric region; and we commonly expect to discover, by examination, some induration in the region of the pylorus. All these symptoms, however, admit of considerable varieties. Indeed, with regard to organic diseases of the stomach in general, I think we are too much in the habit of expecting the symptoms to be perfectly uniform; and of concluding upon this principle, that no organic disease exists when there are considerable intermissions of the symptoms. Attention to the history of these diseases will satisfy us of the fallacy of this principle—by showing us the many varieties of symptoms which occur in connexion with organic affections of the stomach, and the remarkable intermissions of the symptoms which may occur even at a very advanced period of them.

From these remarkable intermissions in the symptoms, while organic disease was present, and advancing to a fatal termination, the following case is of some importance.

Case VI.—A gentleman, aged 30, was for several years liable to paroxysms of pain in the stomach. The pain usually continued for several hours, and went off with vomiting, and it returned at uncertain intervals, frequently of many weeks. Upon several occasions, he appeared to have got entirely free from it. He was in other respects in good health, except being considerably troubled with hæmorrhoids, until about a year before his death, when he was suddenly seized with copious vomiting of blood. This soon went off, and in a few days he was in his usual health; but from this time his former complaints rather increased. The attacks of pain, followed by vomiting, were more frequent, and he was again several times attacked with vomiting of blood, but still he had considerable intervals of health; no hardness could be discovered by examination, and that uniformity of symptoms was entirely wanting, which usually accompanies organic disease. His death was rather unexpected. After having complained for two days of pain in his stomach in the usual form, he was found, in the morning of the third day, exhausted and without pulse, and died in a few hours. Three days before his death, he had been able to walk out a good deal, and made no particular complaint. *Dissection.*—The pylorus was surrounded by a mass of scirrhus, the size of an orange, very firm, nearly cartilaginous. Among the intestines there were extensive adhesions. The body of the stomach, the liver, spleen, and pancreas, were healthy. There was some ossification of the valves on the right side of the heart, and the right auricle was remarkably distended with coagulated blood. The lungs were healthy.

In the following case, the symptoms were more distinctly defined, but without any organic disease to be discovered by examination during life.

Case VII.—A gentleman, aged 66, came under my care only a few weeks before his death. He was then emaciated in an extreme degree, with an exhausted withered look. He had been long in bad health, but particularly for the last four months, during which period he had been affected with frequent vomiting, though not occurring at any regular periods. When questioned about it, he said that he seemed to vomit spontaneously, to relieve an extreme uneasiness which took place in his stomach; and accordingly, upon putting him upon a regulated diet, it appeared that he could in a certain degree prevent it. His debility and emaciation however continued to increase, and he died in a state of extreme exhaustion, in June 1817. No organic disease could be discovered on examination; and for some weeks before his death, the vomiting had in a great measure subsided. *Dissection.*—A mass of scirrhus surrounded the pylorus, the size of a small apple, and projected in the form of papillæ into the cavity of the stomach. The principal projection of the mass was backwards, where it had formed adhesions, by means of which the pylorus was firmly bound down to the pancreas. The stomach was in other respects sound, and the other viscera were healthy. The pyloric orifice was not so much contracted as it usually is in such cases, for it admitted the point of the thumb.

The following case was still more remarkable from its progress, and the absence of all the symptoms which usually characterize the disease.

Case VIII.—A man, aged 40, came under my care in December 1817. At that time he was weakened and emaciated to the last degree, with a weak pulse at 120, but without any other complaint; he had no pain and no cough; his appetite was good, and his bowels natural; and the functions of the stomach were entirely healthy. About half way between the ensiform cartilage and the umbilicus, a hardness was felt, which could be traced for several inches, and was painful when pressed. He had been ill 18 months, and the affection commenced with vomiting, which occurred generally five or six times a day. This continued for five or six months, and then ceased entirely; and for the last twelve months he had had no complaint except progressive debility and emaciation. He died completely exhausted in the beginning of February, having continued without any other symptom except repeated attacks of violent pain in the abdomen. There was no vomiting, and the appetite had continued tolerable, and the bowels regular. *Dissection.*—A large mass of scirrhus, four or five inches in extent, surrounded the pylorus, and the pyloric orifice was so narrowed as scarcely to admit the point of a very small finger. The inside of the mass opened upon the inner surface of the stomach, by an ulcerated surface, covered with large cancerous-looking tubercles. The other parts of the stomach were tolerably sound, and the other viscera were healthy.

These cases present to us phenomena of considerable interest in the pathology of disease of the pylorus; while they show the existence of it, characterized by gradual emaciation, without any urgent symptom, or by symptoms which suffered such remissions as did not appear to correspond with the existence of organic disease. Nor are they, in these respects, singular exceptions to the general history of the disease: for many cases are on record, which show the affection existing, and of great extent, and with remarkable varieties in the symptoms. A man mentioned by Chardel was affected with a strong pulsation in the epigastric region, in which a pulsating tumour was felt corresponding with the pulsations of the heart. It was most troublesome when his stomach was full; but his appetite was good, and the functions of the stomach were unimpaired. He became, however, gradually exhausted, and died without any other symptom, except diarrhœa and œdema of the legs. On dissection, the tumour was found to be an enormous mass of scirrhus, occupying the posterior part of the pylorus, and extending along the small curvature of the stomach. The pancreas also was hard, and the liver tubercular. A woman mentioned by the same writer, had pain in the epigastric region, followed by jaundice, and this by ascites. The jaundice became very deep, with great wasting and loss of appetite; and she died of extreme emaciation, with diarrhœa. On dissection, there was found scirrhus of the pylorus, and of the pancreas; and the latter compressed the ductus communis; the other viscera were healthy. In another case, which was complicated with enlargement of the liver, the patient died, gradually exhausted by violent pain in the epigastrium, which was rather relieved by taking food; he had a feeling of constriction about the throat, with dyspnœa and dropsical symptoms, but no vomiting through the whole course of the disease. A mass of scirrhus, three inches in length, occupied the pylorus, and extended along the small curvature of the stomach.

It is indeed to be kept in mind, that in these affections, though there be a considerable mass of scirrhus existing on the outside of the pylorus, there may be very little contraction of the pyloric orifice; but it does not appear that this circumstance regulates these peculiarities of the symptoms. In Case VII. of this paper, the contraction was but small, while the symptoms were little different from those of the most common forms of the disease; and in a case by Chardel, the pyloric orifice did not appear to be contracted at all, though a large mass of scirrhus surrounded the pylorus. In this case, there was frequent vomiting, though not at regular periods, with violent pain in the epigastric region, febrile paroxysms, and rapid emaciation; and in another case, in which the pylorus was very much contracted, the vomiting was not regular, but frequently ceased for several days at a time. A man, mentioned in the Journal de Médecine for October 1815, had loss of appetite, dyspeptic symptoms, gradual emaciation and pain in the left side of the abdomen, betwixt the umbilicus and the spine of the ilium. Nothing could be felt by examination in the seat of the pain; but, after two months, a hardness was discovered in the region of the stomach. The disease proved fatal in another month, without any urgent symptom but progressive emaciation and slight vomiting for three days before death. The stomach was found thickened through nearly its whole extent, and adhering to the neighbouring parts. The pylorus was imbedded in a mass of scirrhus, and its orifice was nearly closed. The liver was enlarged; the other viscera were healthy.

We have seen, farther, that the disease may exist, and go on to a fatal termination, without being discovered by examination during the life of the patient. This may result from several causes. The projection of the mass may be chiefly on the posterior part, and it may be bound down by adhesion, as in Case VII., so as to keep it beyond the reach of contact. But, besides this, the disease may be of so small extent as could not possibly be discovered in this manner, while it is capable of producing the usual symptoms, in their most severe form. In a case mentioned by Dr Morison, * the pylorus was almost totally obstructed by small tubercles arising from the mucous membrane of it, without any external disease. The patient died, after several years illness, with pain of the stomach and vomiting; and in a case by Chardel, with the same symptoms, the pyloric orifice was reduced to a very small chink, but with very little external enlargement; consequently, nothing had been discovered by examination during life, though the patient was very much emaciated.

Symptoms similar to those which usually accompany scirrhus of the pylorus, may arise from induration and thickening of other parts of the stomach, especially if the disease be of the tubercular kind. Of this I only add the following example; others are mentioned by Chardel and Morgagni.

Case IX. A woman aged 56, (August 1816), had been liable for about a year to disorders of the stomach, distention, acidity, indigestion, and occasional attacks of acute pain. After several months from the commencement of these symptoms, she began to be affected with vomiting; and for the three or four last months, had vomited daily, generally in the afternoon or evening, at irregular periods after

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* Med. Obs. and Inq. Vol. VI.

dinner. Sometimes she escaped it till she went to bed, but then it always came on early in the night. A hard tumour of considerable extent could be felt in the epigastric region. She died extremely emaciated in September, having before her death discharged much blood, both by vomiting and by stool. *Dissection.*—The stomach adhered extensively to all the neighbouring parts, so that it was with difficulty separated from them. The cardia and pylorus were healthy, and a small portion of the stomach adjoining each of these openings. The whole of the smaller curvature, except these two small portions, presented one continued mass of scirrhous hardness, in most places an inch in thickness, and when cut into, white and very firm. On the inner surface of this portion, about half way betwixt the two orifices, there were two tumours, one the size of a pigeon's egg, the other of a hazel nut. Externally, they were of a dark purple colour; internally, white. The large curvature and the anterior part of the stomach were extensively ulcerated, of a dark colour, and very irregular thickness. At one place there was a perforation, from ulceration, the size of a shilling. The pancreas was hard, and the liver tubercular. The other viscera were healthy.

An important practical conclusion from many of the facts that have been mentioned is, that symptoms which have merely the characters of dyspepsia, may be connected with extensive organic disease, and that the most careful examination should always be made in such cases, especially if the dyspeptic symptoms are accompanied by much emaciation. Even when no tumour or induration can be discovered by such examination, the feeling of an unusually strong pulsation in the epigastric region, is a symptom deserving much attention, as it is often produced by a solid body interposed betwixt the hand and the artery, though the induration may not have attained such a size as to be itself discovered by the examination.

On the other hand, it is an important fact, that long continued vomiting, occurring so periodically as to have every appearance of extensive disease, is sometimes connected with slight causes. Of this I have met with the following remarkable example.

Case X. A lady aged between 60 and 70, had been affected for more than 30 years with periodical vomiting, which returned so regularly a few hours after meals, that during the whole of that period, she had vomited a part of almost every meal. It was brought up without nausea, or any unpleasant effort; and the affection had never produced any bad effect upon the general health. I saw her occasionally for several years, and she continued to enjoy good health until she began to fall off rather suddenly; and died after a short illness with rapid failure of strength and diarrhœa. On dissection, the only morbid appearance that could be discovered by the most careful examination, was a small tumour like an enlarged gland, the size of a hazel nut, or a very small walnut. It lay in contact with the outside of the stomach, near the pylorus; and, in other respects, all the parts were perfectly healthy.

In a similar case by Morgagni, in which the symptoms had existed for 24 years, the only morbid appearance that could be detected, was a slight induration of the pancreas. Many years ago, I examined the body of a woman who died, gradually exhausted by daily vomiting, which had continued more than a year, and could discover no morbid appearance except the gall-bladder distended, with a great number of biliary calculi, which completely filled it.

Part III.

DISEASES OF THE PANCREAS.

Case XI.—A man, aged 56, had been in bad health for about two years, with obscure symptoms of a dyspeptic kind. He had some pain in the left hypochondrium extending into the back, with oppression of the stomach, indigestion, and gradual emaciation, and died gradually exhausted, without any other symptom, except that, for a week or two before his death, there was a considerable degree of jaundice; he never had any vomiting, and his bowels were easily kept open. No disease could be discovered by examination during life,—even after he became emaciated to the last degree. *Dissection.*—The stomach and intestines were healthy. Behind the stomach, in the seat of the pancreas, there was a morbid mass, four or five inches in breadth, and somewhat less in thickness. It was closely attached to the spine, and surrounded the aorta. It varied in its structure, some parts being hard, almost cartilaginous, others softer, and composed internally of alternate layers of yellowish and white matter. The liver was somewhat enlarged and soft. The other organs were healthy.

Case XII.—A woman, aged about 40, was affected with slight uneasiness in the region of the stomach, and frequent vomiting. The vomiting gradually increased in frequency, until she vomited almost every thing she took into the stomach, and she died, gradually exhausted, without any other prominent symptom, after the vomiting had continued about a year. A remarkable circumstance in this case was, that though she died with gradual and progressive loss of strength, there was no emaciation; but on examining the body, a coating of fat two inches in thickness was cut through in opening the abdomen. *Dissection.*—The pancreas was found in a state of scirrhous hardness, without much enlargement. No other morbid appearance could be detected in any of the viscera.

Case XIII.—A young man, aged 16, a printer, in May 1812, began to complain of pain in the region of the stomach, extending through to the back. It continued, and rather increased, but without confining him from his usual employment, until July, when he began to be affected with vomiting, which generally occurred two or three hours after dinner. He commonly retained his breakfast. In September, when I first saw him, he vomited every thing. He was then much wasted; and a large irregular tumour was distinctly felt in the epigastrium, which was painful when pressed. He took a good deal of food, but retained nothing. His strength sunk gradually, and he died in the end of December. For eight or ten days before his death, the vomiting ceased, and he was then affected with severe diarrhœa. *Dissection.*—The pancreas was enlarged, so as to form a mass seven or eight inches long, about five inches broad, and about three inches in thickness. The stomach, the duodenum, and the arch of the colon, had formed adhesions to the mass; and the stomach seemed a little thickened in its coats. The other viscera were healthy.

Many cases are on record of chronic diseases of the pancreas; and they exhibit the same diversity of symptoms which occurred in the examples now described, and nearly in the following proportion. Of twenty-seven cases, which I find described by various writers, six were fatal, with gradual wasting, and obscure dyspeptic complaints, without any urgent symptom. In eight, there was frequent vomiting, with more or less pain in the epigastric region; and thirteen were fatal, with long-continued pain, without vomiting. In some of these, the pain extended to the back; and in others, it was very much increased by taking food. In several of the cases there were dropsical symptoms; and in three or four there was jaundice, from the tumour compressing the biliary ducts. In the morbid appearances also there was great variety; the pancreas being, in many of the cases, much enlarged; in others, in a state of scirrhous hardness, with very little enlargement. It does not appear that any distinct relation can be traced betwixt the urgency of the symptoms and the degree of enlargement, for it existed in a great degree in some of the cases in which the symptoms were slight and obscure; and there was hardness, with little or no enlargement, in others, in which the symptoms were defined and violent.

Inflammation of the pancreas seems to be a rare disease; but several cases are on record in which it was found suppurated and gangrenous. The symptoms do not appear to have been distinctly defined. There was generally pain, which was chiefly referred to the back—while in some it had more the appearance of colic pain. Vomiting occurred in a few of the cases; but it does not appear to have been a common symptom. In a man mentioned by Barbette, who died with urgent vomiting after a short illness, the only morbid appearance mentioned, was a sphacelated state of the pancreas. The same appearance occurred in a man mentioned by Greisel, who had been liable to colic pains, and died rather suddenly, having complained only of a feeling of internal coldness. Abscess of the pancreas was observed by Tulpius and Bartholinus. In two cases by the former, it was connected with quartan fever; and in a case of continued fever, in which there had been much pain in the back, Guido Patin found an immense abscess occupying the whole of the pancreas. Portal found a complete suppuration of the pancreas in a man who died suddenly after two or three attacks of vomiting, followed by syncope. He had previously suffered from a paroxysm of gout, from which he was supposed to be convalescent. The same writer found the pancreas softened and gangrenous in a man who died with obscure pain in the abdomen, accompanied by wasting and occasional nausea and diarrhœa. A gentleman mentioned by Dr Percival,* had jaundice and bilious vomiting; a tumour appeared at the epigastrium, his strength failed. Blood and fetid pus were discharged by stool, and he died gradually exhausted in three months. The pancreas was found greatly enlarged, and contained a considerable abscess. The ductus communis was obliterated from the pressure. Calculous concretions have been found in the pancreas. De Graaf, found seven or eight calculi the size of small peas, in the pancreas of a man who had been long liable to vomiting and diarrhœa, and died gradually exhausted at the age of thirty. Portal found the pancreas much enlarged, and containing twelve calculi, some of them the size of nuts, in a man who died of a disease of the aorta.

The ancients attached much importance to the pancreas, as the seat of many obscure diseases; but it does not appear that their opinions were founded on any accurate observations. There is much reason however to believe, that diseases of the pancreas have a most important influence upon the functions of digestion and assimilation, and may produce, in this manner, very serious effects upon the system, while the symptoms immediately arising from the disease are so slight and obscure, as not to indicate what organ is affected. It seems probable that this is one origin of the remarkable affection that has been called Anæmia. I have been led to this conjecture by several observations, particularly by the following remarkable case which I saw some time ago, along with Mr Newbigging.

CASE XIV.—A gentleman, aged 35, died, after an illness of about eighteen months duration, in which it was, to the last, impossible to say what was the seat of the disease. His complaints began in London with a febrile attack, which left him very weak. From that time he was liable to dyspeptic symptoms, with variable appetite, and undefined uneasiness in the epigastric region. Without any other complaints, he gradually lost flesh and strength ; and when Mr Newbigging saw him, in January 1822, he found him thin and weak ; but he was particularly struck with his remarkable paleness, even his lips, and the inner surface of his mouth, being entirely colourless. About this time he had some vomiting, and was feverish for a day or two ; but these symptoms soon subsided, and left him, as before, with variable and capricious appetite, and an irregular state of the bowels, which were sometimes costive, and sometimes loose. He had frequently perspirations in the night-time, and appeared at all times languid and faint ; but his pulse was natural : he took a good deal of food ; and there was no symptom that accounted for his exhausted appearance. In January, he seemed to improve considerably ; but in February he became worse, being affected with diarrhœa, and his urine became scanty, and deposited a mucous sediment. These complaints, however, soon subsided ; and he then complained chiefly of throbbing in the head, and a constant noise in the left ear. When I saw him in the middle of April, he was reduced to the last degree of paleness and debility ; but his pulse was full, strong, and regular. He took a good deal of food, and complained of nothing except a painful pulsation in his left ear. He died in the end of April, without any change of the symptoms, except that his pulse became frequent a few days before his death ; but it continued full, and rather strong. The action of the heart was rather violent, and seemed to produce throbbing over the whole body.

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* Trans. of the King and Queen's Colleges, Vol. II.

On dissection, all the internal parts were found remarkably pale and void of blood. The heart was sound, but remarkably empty. The pylorus was thickened, and firmer than natural, and had contracted an adhesion to the pancreas. The pancreas was considerably enlarged, and was diseased through its whole structure, being in general hard and cartilaginous ; but in some places soft, with the appearance of the medullary sarcoma. All the other organs were healthy.

(To be continued.)

II.

On the Circulation of the Blood in the Head. By JAMES CARSON, M. D. Liverpool.

THE general agents which are employed in the circulation of the blood, and which themselves constitute a part of the animal machine, are the lungs, the heart, the arteries, and the veins. There are other agents which may be called general, but which do not peculiarly belong to the animal fabric. The chief of these are atmospherical pressure and gravity.

But, in addition to these, there are other powers or contrivances which may be called subsidiary, and of which the office is confined to particular parts of the circulation; among these may be ranked the machinery of the liver and the spleen.

The observations which I am now about to make will be confined to the subsidiary agents, the office of which belongs extensively to the motion of the blood through the encephalon; and, in noticing these, it will be necessary to consider, generally, this part of the circulation.

The cranium is a hollow spheroid constructed of bone. Though the shell of this spheroid be formed of several bones, yet, in consequence of the exact and perfect manner in which these bones are united by what anatomists term sutures, it may be considered to be composed of a single bone continuously of the same structure.

This osseous crust, when examined after death, and when all the softer parts have been separated, exhibits many small perforations, affording, in the sound state of the parts, a passage for nerves, arteries, and veins, besides one much larger, by which a communication is formed between the contents of the cranium and those of the spinal canal, a cylindrical tube of the same osseous structure, and perforated in the same manner, and for the same purposes, as the cranium itself.

As a foundation for the arguments which may be advanced in this inquiry, it seems necessary to examine briefly the component parts of the encephalon, beginning with the vascular and membranous parts of it.

The arteries, at their entrance into the cranium, through the perforations appropriated to that purpose, possess the same structure with those vessels in other parts of the body. They are described as taking at this place a winding or tortuous course, which is supposed to retard the impetuosity of the blood when it is too violent. Here, as in other places where they perforate, they form, by their external coat, an adhesive communication with the membrane, which, in this case, is described as a continuation of the dura mater lining the passages.

After they have entered the cranium, they for some time run in grooves formed on the internal surface of the skull, sending off, as they advance, branches to the brain and membranous parts of the encephalon. The arterial system in the head seems to differ little from the same system in other parts of the body. The capillary arteries of the head communicate with the capillary veins, in all probability after the same manner in which these vessels communicate in other parts of the body, and which is at present altogether unknown.

The part of the venous system within the head, intercepted between the ultimate terminations of the arteries and the sinuses, is in no respect peculiar or different from the same system in other parts of the body. After this, a remarkable change in the venous structure takes place. The veins do not unite to form large branches or trunks of the same structure with themselves, but open, as is well known, into vessels of a peculiar fabric, termed by anatomists sinuses. These sinuses it will be necessary briefly to describe.

The internal surface of the cranium is lined by a strong membrane, termed the dura mater, composed of two plates, and adhering throughout to the skull.

Foldings of the internal plate of this membrane cut the cranium both vertically and horizontally, separating the brain into two divisions or hemispheres, and the brain, properly so called, from the cerebellum. The two plates of which these foldings of the internal lamina of the dura mater are formed, adhere firmly together, excepting at their approach to the skull, where they separate and become attached to its internal surface at a distance from each other. The space formed by the separated plates of the dura mater, and the part of the cranium intercepted between the lines of the attachment of these separated plates, are the sinuses. Their transverse section forms a triangle. In consequence of the great strength of the membrane forming the sinuses, and of the stretch upon which it is always kept, the membranous sides of the sinuses preserve their position in opposition to any moderate force directed either against their internal or external surfaces. They are vessels whose dimensions never vary. They are very numerous; are situated in all quarters of the head; they differ greatly in size; and have each a communication with all the rest, either mediately or immediately. Such are the vessels into which the veins, returning the blood from the brain and its membranes, discharge their contents. The sinuses, by passages through the cranium, communicate with veins of the neck; and, at this part of the sanguiferous circle, the ordinary venous structure is again restored. As these vessels pass through the cranium, their external surface forms a circular adhesion to the membranes lining the bony canals through which they pass.

With respect to the nerves, the other system of vessels of the head, it is only necessary to remark, that they also, at their exit from the cranium, form a similar attachment to that of the blood-vessels to the periosteum, lining the door of their exit. Besides the vessels now enumerated, there belong to the encephalon other receptacles which now require to be considered, and which will be found to hold an important place in the present investigation. I allude to the ventricles. The ventricles of the brain may be described as an irregular fissure, traversing the interior substance of the brain, varying greatly in breadth, and dilatable at different places, according to circumstances, into cavities of greater or less extent. The more extensively dilatable parts of this fissure have received the name of ventricles; and the narrow channels by which they communicate, have received the name of passages, or roads between the ventricles. The ventricles are generally reckoned to be four, and each communicates with all the rest, either directly or indirectly. These ventricles, when dilated, so as to form cavities, contain a watery fluid. It is still a matter of dispute, whether they are at any time altogether empty, or whether their opposing surfaces are not at all times, and at all places, separated by a portion of water. The water, which may exist in the ventricles, will, during the sound state of the parts, be distributed in a given ratio through all the ventricles. This seems necessarily to follow from the position of different parts of the brain being fixed. If a section were made to pass through the brain, taking in its course the ventricles, while the other parts remained in the relation in which they exist in life, this section would exhibit a surface, on which the ventricles and communicating passages might be compared to the lakes and rivulets upon a map.

The contents of the cranium may be divided into three parts. 1. The blood contained in the arteries and veins. 2. The water in the ventricles. 3. The parenchymatous substance of the brain itself. The membranes, the coats of the blood-vessels, and nervous substance, we propose, for the sake of argument, to place in the last of these divisions. As the limits of the cranium are fixed, and as the solid contents of it must be at all times the same, it is evident that the proportions of any of these divisions cannot vary without affecting the proportion of one or both of the other two. As the substance of the brain, during life, cannot undergo any considerable alteration suddenly, or without the operation of a slow process, and as this substance is incompressible, in a manner to be afterwards explained, and as the quantity of water cannot be instantly increased or diminished, and is itself also incompressible, it follows, that the quantity of blood contained in the head must, on the supposition of the other two divisions of the contents of the cranium remaining unchanged, be at all times the same. Let it be supposed, that the quantity of water contained in the ventricles, and the quantity of brain, two of the three constituent portions of the contents of the cranium, remained unchanged, the quantity of blood, the remaining portion, could not be augmented by any force by which the cranium was not ruptured, or the cavity enlarged. No blood, in these circumstances, could be thrown into the head by the arteries, unless an equal quantity was at the same instant discharged from the cranium through some of its outlets. As the contractions of the arteries by which blood is thrown into the head, are synchronous, no blood could be returned back from the encephalon through one artery, at the moment that a quantity was thrown into it by another. The sinuses are the only outlets through which blood could be discharged. But, by what means can any blood be displaced from the sinuses? If we consider for a moment the form and structure of these vessels, and the oblique manner by which they are perforated by the small veins leading from the brain and membranes, it is impossible to imagine that the effect could be accomplished by a force directly communicated from the heart and arteries, in the manner of a *vis a tergo*. But the system is provided with other agents by which the blood in the sinuses can be affected. These are gravity in certain positions of the head, and another still more effectual and permanent, the removal of a portion of the pressure of the atmosphere from the blood at the ends of the sinuses, communicating with the veins exterior to the head, by the dilatation of the heart and the resilience of the lungs constantly tending to increase the diameter of the veins within the chest. But this class of agents, if not so powerful as to outbalance the whole weight of the atmosphere, or if the parietes of the cranium did not yield, could not withdraw a single particle of blood from the head, unless a quantity equal to that to be abstracted, was at the moment supplied by the arteries. But the abstracting powers, though singly insufficient to move the blood in the head, being permanent, are at all times ready to cooperate with the successive injections of the arteries. By the joint agency of these two classes of powers, an impulse is given to one end of a column of fluid, and an abstraction of resilience, or, in popular language, a suction is applied at the other; and thus an easy, equable, and duly proportioned motion is given to all the blood contained between the first entrance of the arteries into the cranium and the outward ends of the sinuses; that is, to the whole blood contained within the cranium.

I do not know of any machinery with which this, for circulating the blood in the head, can in all its parts be compared for the sake of illustration. The heart and sinuses resemble, in some degree, a succession of pumps, by which water is raised to one elevation, and then from that to another; with this difference, that it is on account of the great elevation to which the water is to be raised that a second pump is required; but, in the case in question, the sinuses, or a first order of pumps, are not required on account of the elevation, but for the purpose of collecting the blood through a thousand channels from all parts of the brain, and of forming reservoirs to which the action of the second order of pumps can be more effectually applied.

The sinuses, and the veins of the neck, have been supposed to form a syphon. But it cannot be by this machine that they act at all times, at least in conveying the blood from the head. In the erect position of the head, they may form a syphon; but, in the recumbent, and, still more, in the dependent state of that part of the body, gravity, which is the cause of the motion of a fluid through a syphon, ceases to influence, and ever opposes the motion of the blood from the head; and the actual removal of a part of the pressure of the atmosphere from the blood on the side towards the heart, or some other equally effective power, is required. But no such power has been exhibited in any of the views taken of the circulation of the blood, excepting in that in which the elasticity of the lungs, and the automatic expansion of the heart, are supposed to have so great a share. Gravity, as has just been stated, may, in the erect position of the head, assist in returning the blood from that part of the heart. The cooperation of gravity with the other agents which have been mentioned, is in this case direct. But even in the pendent and horizontal position of the head, gravity, though opposed to the motion of the blood in the jugulars, has indirectly a considerable effect in securing an uninterrupted and free motion in these vessels; for, by the lateral pressure which it in these circumstances gives to the sides of the veins, gravity prevents what may be termed a collapse of these vessels, which, if it took place, would intercept the suction influence, and, by that means, it more than counterbalances the opposition which it directly supplies to the return of the blood.

A singular phenomenon, exhibited in the motion of the blood from the head, receives a striking illustration from the views which have now been exhibited, and appears to me singularly confirmatory of their truth, and may with propriety be noticed at this place. The veins of the neck, particularly the jugulars, vessels which receive the blood from the encephalon, and the pectoral cava, into which they discharge their contents, contrary to the usual mechanism of the veins, pulsate. For the want of a better explanation, this effect has been usually attributed to the stroke communicated by the accompanying arteries to them, and transmitted by these vessels in a secondary way to the touch. The arguments by which the opinion of the pulsations of veins being attributable to accompanying arteries is, I think, refuted, have been stated elsewhere, and need not be repeated here. The simple, and, I trust, satisfactory explanation about to be given of this phenomenon, is this. The arteries, which convey the blood to the head, throw this fluid into the cranium by synchronous jets. An equal quantity, according to the hypothesis contended for, must at the same instant be discharged from the cranium into the veins. This must also be by jets, which cause successive currents, or, in other words, pulsations in these veins. While the contents of the ventricles, and the substance of the brain, remain unaltered, it is evident, that the quantity of blood contained in the cranium must be at all times the same. No force could, in the state supposed, increase this quantity of blood; and from this circumstance a great security is given to the vessels of the brain, the rupture of any of which would be attended with fatal effects. Nor would the abstraction of any quantity of blood from the rest of the system, diminish that contained within the cranium. This consequence, which is so clearly deducible from the principles which I have attempted to establish, has lately been confirmed by the observations of an ingenious and able writer. Dr Pring bled a number of animals to death, and found the blood-vessels of the head all fully distended with blood. Although the quantity of blood contained within the head be at all times the same, in the circumstances stated, the distribution of it may vary. One part of the encephalon may at one time contain a greater quantity of blood than its ordinary proportion; but some other part, in that case, must contain less. In what circumstances this occurs, or what are the consequences of it, are questions which, I suspect, are at present unanswerable. A partial inflammation of the brain, if inflammation of that organ be accompanied with the same phenomena accompanying inflammation in other parts of the body, must alter the distribution of the blood in the cranium. Blood would be accumulated in the inflamed part, and there would be a corresponding abstraction of it from another. From which of these states greater danger would arise, is altogether unknown. Though the quantity of blood belonging to the encephalon must, in the state supposed, be at all times the same, the quantity which passes through it in any given time will vary according to the velocity of the blood.

We have hitherto been considering the motion of the blood in the head in the case in which the two other constituent portions of the encephalon are supposed to remain the same. We are next to examine how far the motion of the blood in this part of the circulation may be affected by any changes in the quantities of one or both of these other portions of the contents of the cranium, by their augmentation or diminution; and, first, with regard to the substance of the brain itself.

The late Dr Alexander Monro of Edinburgh contended, that he had proved by experiments, that the substance of the brain was incompressible. What that learned Professor meant by the incompressibility of the brain, may require some explanation. If we take a piece of the brain into our hands and squeeze it, few substances can be found more yielding and compressible. But the meaning of the term, as employed by the Professor, was, that the ultimate particles of which the brain was composed were incompressible, or that the same number of these particles would, under any different degrees of pressure, occupy the same space. Perhaps the idea may be expressed in other words, by saying, that the substance of the brain was inelastic. In this light, the muscular substance, and indeed most other parts of the body, may be considered incompressible. Previously to the time of Dr Monro, and by many physicians of the present day, in the case of accumulation of water in the ventricles, the water was supposed to compress the brain into smaller bulk. Dr Monro justly contended against this opinion, and maintained, that the water, in these circumstances, by pressing against the brain, caused the absorption of that organ; and that, by this means, a part of the substance of the brain was consumed, or rather dislodged from the encephalon, to give room for this increased quantity of water. An opinion was held by Dr Monro, and indeed by all the physiologists of his day, that the office of absorption was solely performed by a peculiar class of vessels called lymphatics. Though the search had occupied the eager and ambitious toil of anatomists during the last fifty years of the 18th century, no lymphatic vessels could ever be found in the brain. It was concluded from this, that water, when once deposited in the ventricles, or in any part of the encephalon, must remain there, as there existed no machinery by which it could be removed. In the case of the disease called Hydrocephalus, Dr Monro was induced to recommend as the only, but awful alternative, the discharging of the water by the puncture of the brain. By what machinery the substance of the brain could be carried out of the encephalon, while there existed none for the removal of water from the ventricles, neither this eminent physiologist, nor any of his followers, have attempted to explain.

But the doctrine, that the lymphatics are the only vessels endowed with the faculty of absorbing, or of taking up fluids deposited in cavities, has been disproved. By far the greater part of the process of absorption, it is now admitted, is performed by veins. Indeed, the lymphatics, to which the sole office of absorption was attributed, owe, in a great measure, the power which they possess, in this respect, to their connexion with the veins within the chest, where the great lymphatic trunk is subjected to the exhausting powers derived from the elasticity of the lungs and the automatic expansion of the heart.

The important physiological truth, that veins are fitted to absorb, is plainly deducible from the causes by which the blood is moved in its passage through the veins; and that they do absorb, has been fully established by the experiments of many physiologists. We are now, therefore, happily relieved from the disheartening conviction that water, when deposited in the ventricles, can never be removed by any natural process. There can exist no doubt, also, that the brain, like every other part of the body, is perpetually undergoing a process of renovation; that its substance is subject to increase and diminution; that it may be augmented under the enjoyment of luxuriant health, as well as wasted by defective nourishment, or long consuming disease.

Let it be supposed, from some cause, that the expending process has been more active than the renovating; and that, in consequence, the substance of the brain has been considerably diminished. In this case, the quantity of one or both of the two other constituent portions of the encephalon must be affected. The space that would otherwise have been a void, must be filled up by an augmentation of the quantity of one or of both of these encephalic portions. Either a greater quantity of blood must be contained in the vessels of the head, or the quantity of water in the ventricles must be augmented, or both of these effects will, to a certain extent, be produced. The last of these conditions is, I believe, the one most usually found. On the dissection of bodies reduced to great emaciation by disease, the vessels of the head are found to be turgid, the substance of the brain to be soft, and to contain an unusual quantity of blood, and the ventricles to be greatly distended with water. Sometimes, in such cases, the quantity of water contained in the ventricles is very considerable; ten ounces is by no means uncommon. Suppose there had existed no receptacles for water, such as the ventricles, and the brain to have been wasted to that degree, by which room was afforded for the admission of ten ounces of water into the encephalon, the blood-vessels of the head must necessarily have been loaded with ten ounces of blood, in addition to the quantity which they already possessed, and by which they appear to have been already too much distended. Long before they could have been distended to the capacity necessary for the admission of so great a quantity of blood, their coats must have given way, and a fatal hemorrhage ensued; or, at all events, they must have been too much surcharged, for the performance of their functions. Hence, we readily perceive the important uses of the ventricles. By becoming the receptacles of a mild fluid, they, in certain circumstances, prevent the blood-vessels from being over distended. By their greater or less expansion, they become the grand regulators of the circulation of the blood through the head. Water in the ventricles, in such circumstances, instead of being considered a disease, is, in reality, the great remedy provided by Nature for the preservation of life, in situations in which it could not otherwise exist. It is the defence set up by Nature for the protection of the breaches or weak points which may exist in this part of her works.

The ventricles of the brain, in consequence of their irregular course, are admirably situate for enabling the substance of the brain to assume that variety of position necessary, as circumstances alter, to give due support to the vessels of the head, without sustaining at any point a disproportionate distention. But, to perceive this sufficiently, the brain itself must be examined.

These uses of the ventricles are most important, necessary, and, as now described, so certainly true, as, if I am not singularly blinded, to require only to be announced to gain assent and acknowledgment of every unprejudiced inquirer. No physiologist has hitherto, I believe, even ever ventured to form a conjecture respecting the purposes for which the ventricles were designed by Nature. The demonstrator generally passes them over with the observation, that they must be of some use, as Nature never forms any thing in vain. That the ventricles may serve, besides those now explained, other purposes at present unknown, is not improbable; for Nature, a rigid economist of her means, often accomplishes various ends by the same machinery.

We have been considering the state of the brain, in which its substance is unduly diminished. Let us notice, for a moment, the opposite condition. Little water will, in this case, find admittance into the ventricles; and if the substance goes on to augment, the space assigned for the blood-vessels will be encroached upon; and if the growing process continues, sufficient room for the transmission of blood in proper quantity will not be left. I think it extremely probable that some of the species of apoplexy and of palsy are the consequence of an overgrowth of the substance of the brain. When no water is found in the ventricles, the substance of the brain will also be observed to be unusually firm and solid, showing little appearance of blood upon being cut into, and the vessels on its surface slightly distended.

As the quantity of blood distributed to the head may be supposed, from the caliber of the vessels which supply that quarter being fixed, to be nearly the same, if, from an overgrowth of the brain, the quantity of this blood transmitted through the encephalon be diminished, a greater quantity must be sent to the parts of the head exterior to the cranium. Hence that redness of the face and eyes, which so frequently attend great obesity, and which is so often the precursor of the apoplectic stroke.

In the cases which, in this head of the inquiry, have been supposed, as well as in all the former, it is very evident that the abstraction of any quantity of blood from other parts of the system, could not diminish the quantity of this fluid within the cranium; and that no arterial force directed against the brain could for an instant place an additional quantity of this fluid within the same limits. Some change in the other two constituents of the contents of the cranium must take place synchronously with any alteration of the quantity of blood in the encephalon. If, however, the substance of the brain, or the water in the ventricles, or the blood itself, be at all compressible, the quantity of blood admissible into the head will be affected by changes in the pressure of the atmosphere. If any, or the whole of the three constituent portions of the encephalon be compressible in the way that has been explained, then a greater quantity of blood will be contained within the head during a high, than during a low state of atmospherical pressure. It is probable that the substance of the brain is in some degree diminished in bulk, as would happen, on the supposition that it was compressible, by an increase in the weight of the atmosphere; for in that state an unusual alacrity of mind and activity of body, as was long ago pointed out by Boerhaave, are enjoyed, while the contrary affections, both of mind and body, are at all times, I believe, to a certain extent, the concomitants of a very low state of the barometer, or a very light state of the atmosphere. If an increased quantity of blood should be admitted into the head during a higher state of atmospherical pressure, the addition would only be as much as was necessary for carrying on its circulation through the head.

It may naturally be asked, from what source is the water lodged in the ventricles derived, and how is the supply of it so regulated, that it should be at all times equal to, and never more than, the demand?

The water is unquestionably supplied from the blood, and is conveyed from those exhalent terminations of the arteries, which, in ordinary circumstances, carry a colourless watery fluid. The mechanism by which the supply is proportioned to the demand, is a subject of considerable intricacy, but may perhaps be thus explained. Let it be supposed, that a certain proportion of the substance of the brain has been by some cause or other withdrawn, without being replaced by a fresh supply, or, in other words, that the quantity of brain has been diminished. The place of the portion of brain which has been withdrawn must be occupied by some other substance; indeed it could not be withdrawn without such a substitution being synchronously made. The augmentation of the quantity of blood distending the blood-vessels, is the direct and immediate manner in which the requisite supply of matter is made. But when the process is continued, the blood-vessels become enlarged to an unnatural degree, and in that case their coats make a resistance to further distention. A force is at this time generated by the resistance of the coats of the blood-vessels. Supposing that the contents of the cranium, as must always be the case, are all equally subjected to the pressure of the atmosphere, and that a force, generated by the resistance of the coats of the blood-vessels, existed, the necessary tendency of this will be, to remove a part of the pressure of the atmosphere from the rest of the contents of the encephalon. What will be the effects of this tendency or effort, when applied to the mouths of the exhalents and absorbents opening into the ventricles? Certainly, to increase the discharge from the former, and to diminish the suction of the latter. The natural consequence is, an accumulation of water in the ventricles, which will go on increasing, until the force generated by the resisting coats of the vessels shall be removed, or until that force shall have been balanced by an equal force, generated by the resistance afforded to further distention by the sides of the ventricles.

If, as may easily be believed to be the case, the coats of the blood-vessels of the brain do at all times make some resistance to their contents, or tend, with some force, to diminish their caliber; in that case, according to the argument now maintained, a certain quantity of water ought, almost at all times, to be found in the ventricles of the brain. Hence, the existence of water in the ventricles of the brains of persons who have been cut off in the enjoyment of perfect health, and the constant absence of adhesions between the sides of the ventricles of the brain, may be explained.

The inquiry has hitherto proceeded on the supposition that the solid dimensions of the capacity of the cranium are fixed; but, if it be pursued under a supposition of a change in these dimensions, either an increase or diminution of them, a new and extensive field opens before us. As I have already, I fear, extended this paper to too great a length, I shall confine the observations which I have to make on this part of the subject within very narrow limits. Any extensive and sudden increase or diminution of the solid dimension of the cranium must necessarily prove fatal, as the circulation of the blood must, in that case, cease before the contents of the cranium can be accommodated to the change; but if the enlargement or diminution of these dimensions be gradual and slow, they may take place to a very great extent, without the destruction of life.

The most frequent instances of the undue enlargement of the cranium occur in infancy. When explaining the structure of the cranium, it was observed that it was constructed of several bones firmly united. In infancy, it frequently happens that the bones are only united by a membranous substance, not yet in a state of ossification. This membrane, from various causes, is liable to rupture or distention; the bones are separated, and the dimension of the head consequently enlarged. To this imperfection of the *crania* of infants may, I think, be traced the great irregularities in the circulation, and the numerous diseases of the head, to which that age is peculiarly liable. If the shell of the cranium give way, and the dimensions of it become enlarged in the manner supposed, the same appearances will take place within the head as were stated to occur under a supposition of a diminution or waste of the substance of the brain. The proportions between the constituent portions of the contents of the cranium, are altered in the same way, though from a different cause. To give to the contents of the cranium that degree of pressure which is necessary for carrying on the circulation of the blood through them, in the manner that has been explained, water becomes accumulated in the ventricles. The blood-vessels of the encephalon at the same time are gorged, and the substance of the brain, though undiminished, becomes soft and flaccid, in consequence of the distention of the blood-vessels running through it. This is one of the species of that frequent disease improperly termed hydrocephalus, or water in the head; for it evidently is not the water, but the enlarged capacity of the cranium, which is the cause of the malady. It is the collection of water in the ventricles which, in these circumstances, place the blood in the head in a state to be influenced by the powers employed for its circulation. The evident remedy for this disease, is to supply the imperfections of the cranium, and to reduce its capacity to its natural and healthy dimensions. Accordingly, Sir Gilbert Blane, physician to his present Majesty, having observed that the disease termed water in the head was always accompanied by a large bregma, or what is called an *open* in the top of the head, sagaciously concluded, that the disease in question was, in some degree at least, attributable to the want of complete ossification of the cranium, and boldly attempted to supply the deficiency by art. This he accomplished by the very simple means of compressing the head by a bandage. The effect of the treatment has been wonderful. It has not only been successful in this eminent physician's own hands; but we every day witness and hear of this disease, which two years ago was deemed incurable, completely removed by this simple, easy, and natural process. It may not be impertinent, perhaps, to remark, that about three years ago, and long before the appearance of Sir Gilbert Blane's paper, I was led to the same treatment, in consequence of the views I had taken of the causes of the circulation of the blood in the head. At this time I was desired to visit a child two months old, a patient of Mr Reay, surgeon, of this place. The disease with which the child was affected, was what is usually termed hydrocephalus externus, and had advanced so far as to leave, in the mind of that experienced practitioner and myself, no hopes of its recovery. The head was greatly enlarged, and increasing daily; a fluctuation was distinctly felt between the parietal and occipital bones; the child was affected with coma. The bringing together the too far separated bones of the head by gentle pressure, and retaining them in that situation by a bandage, was proposed, and the proposal was readily acceded to by Mr Reay, who also applied the bandage. The effect of the treatment was surprising. After the very first application of the bandage, which was daily renewed, the unpleasant symptoms began to disappear, and the child advanced, without interruption, to a state of perfect health, in which it at this day remains.

Fears may be entertained against this practice, lest injury be given by it to the tender structure of the brain. But it must be remembered that the brain, at the time, is in a state of over-distention, and that the pressure is only calculated to restore it to the easy condition that is natural to it. That the brain, in early infancy, may be compressed without injury to life, we have sufficient proof, from the practice of various tribes of Indians, who compress the heads of their infants into a variety of forms, conformable to the false taste of beauty prevalent among them. It is never to be supposed, however, that this remedy is to be applied without the caution which an acquaintance with the subject will not fail to supply.

There is still another state in which the ratio between the brain and the capacity of the cranium may be deranged so as to give rise to disease. Though the ossification of the cranium be complete, its capacity continues to enlarge, and this it may be supposed to do in a quicker ratio than that in which the brain is augmented. If such a condition ever takes place, it would account for those cases of hydrocephalus which occur after the age of infancy, and after the ossification of the cranium has been completed. In such cases, the remedy proposed by Sir Gilbert Blane would evidently be inapplicable. When the cavity of the cranium is diminished by a thickening of the skull, or by any other means, the encephalon will be placed, in situation, similar to that in which it is when the substance of the brain exists in too great a quantity, and therefore requires no further description.

Dr Wilson Philip, in the notice which he has done me the honour to make of my opinions respecting the powers by which the blood is moved, in a paper published in the last number of the Medical and Chirurgical Transactions, thinks that he has discovered an argument subversive of the supposition of the blood being returned through the veins by suction. The objection which he has made is, that the veins, being vessels the coats of which are yielding, are unfit for being the channels for raising blood by suction, as their sides would, upon the application of that power, be drawn together, and the vessels, of course, shut. But the Doctor does not take into consideration, that this objection can only apply to vessels through which a fluid is to be raised of a greater specific gravity than that in which the vessel is placed. If water, for instance, is to be raised to a height through the air by pumping, the stem of the pump must necessarily be strong enough to resist a collapse of its sides, in consequence of the difference between the weight of a column of water and that of the same column of air. But if the water is to be raised through the same medium, that is, through water, then an yielding vessel is sufficient; for the pressure on the internal surface of the tube is equal to that upon its external surface. The pumping of the water through the tube could not, in this case, cause a collapse in the sides of the vessels, if these vessels were once dilated by the fluid in which they are immersed. But, as the animal fabric may, so far as regards the point in discussion, be considered a vessel filled with blood, the veins are immersed in a fluid of the same specific gravity with that which passes through them, and therefore cannot have their sides drawn together by suction originating in the chest, however remote those vessels may be from the heart. The objection stated by Dr Philip had, at the time I published the Inquiry into the Causes of the Motion of Blood, considerable weight with myself; and I conceived and stated, that a share of the power derived from the heart and arteries was extended to the blood in the veins, for the purpose of preventing the sides of those vessels from being drawn together. But I now think that such a supposition is unnecessary. In this opinion I have been the more confirmed, by considering the causes of the vacuity of the arteries after death, when blood is circulated through its whole course by the suction derived from the elasticity of the lungs alone.

I beg leave to take notice of another error into which Dr Philip has fallen in the same paper. He says, that, even according to my own experiments, the elasticity of the lungs of an ox is not greater than is sufficient to balance a column of eight or ten inches high, and fortunately states the page of my work for his authority. It is there stated, that upon inverting the windpipe of an ox into a pitcher of water, and then opening the chest to admit of the collapse of the lungs, when the end of the windpipe was depressed eight or ten inches below the water, bubbles of air appeared upon the surface of the water. I never for a moment dreamt that this experiment could be taken to prove the force of the elasticity of the lungs, but only its existence. From this experiment, the range of that elasticity, or the difference between the length of an elastic fibre at the times in which the lungs are fully dilated and collapsed, may perhaps be calculated, but not the intensity of that force when the lungs are fully expanded; for it must be remembered, that the lungs collapse upon an elastic fluid, which must be compressed to a considerable degree before any bubbles of air could appear on the surface of the water in this experiment. The experiments by which the amount of the elastic power of the lungs of different animals are ascertained, will be found in a paper on the Elasticity of the Lungs, published in the Philosophical Transactions for 1820, where it will be found the elasticity of the lungs of an ox was greater than the weight of a column of water 18 inches high.

III.

Essay on the Motive Powers of the Circulation of the Blood.[*]

By DAVID WILLIAMS, M. D.

No object has engaged more of the attention of the medical world, than the circulation of the blood in health and disease; nor is it to be wondered at, when its great importance in the healing art is taken into consideration. The action of the vascular system is one of the principal indications which enables the physician to determine the nature of the greater number of diseases to which mortality is subject; it is his chief guide in directing his treatment; and, in general, after the vital spark has fled, the only cognizable disorganization which necrotomy developes to him, is the effect of a morbid excitement in some of its branches. Although the Harveian doctrine of the course which the blood describes through our frames be universally admitted, yet, respecting the powers which maintain its motion along that course, the most opposite opinions are entertained by men whose talents claim our greatest admiration. Indeed, neither the part which the heart performs, nor the degree of influence exercised by the blood-vessels, have yet been satisfactorily defined. Therefore, I may be allowed to say, without trespassing on the feelings of my predecessors, who have endeavoured to disclose the secret actions of the same intricate system, that all the theories which I am acquainted with on the subject, appear to me to be somewhat vague. Some writers believe the projectile power of the ventricles of the heart alone to be adequate to the circulation of the blood. Others think, that the arteries contract on the blood by virtue of their inherent irritability, by which they preserve the impulse given to it by the contraction of the ventricles. Again, others, considering the projectile power of the ventricles and arteries inadequate to the propulsion of the blood through the veins, have attributed great importance to the influence of the active power of dilatation inherent in the ventricles, aided by what has been denominated the resilience of the lungs; the heart thus acting as a forcing and a suction pump. As Dr Wilson Philip has very ably examined the merits of the above hypotheses, in a valuable paper which is inserted in the 2d part of the 12th volume of the Medico-Chirurgical Transactions, no further notice will be taken of them in the following pages. In this Essay, we shall first of all glance over the structure and arrangement of the heart, arteries and veins; then we shall proceed with the relation of the phenomena observed during our experimental investigation; afterwards, we shall consider the deductions which they seem to warrant, by inquiring particularly into the influence of the venous system, and into the nature of the power which dilates the cavities of the heart; concluding with a brief exposition of the circulation.

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Mammalia, and birds having a double circulation, have two hearts included in one (enveloped by a membrane, called the pericardium), each consisting of an auricle and ventricle, consequently the human heart has four chambers, a right auricle and ventricle, and a left auricle and ventricle; the two former are actively subservient to the pulmonary or the less circulation, and the two latter to the corporeal or greater circulation. There are valves placed between the ventricles and auricles, which favour the passage of the blood from the auricles to the ventricles, but prevent its return from the ventricles to the auricles. The movement of the ventricles are synchronous, and likewise those of the auricles; when the ventricles are in a state of diastole, the auricles are in a state of systole, and *vice versa.* The structure of the body of the heart is muscular, but about its base it is partly ligamentous or tendinous. The arteries originate in two trunks from the ventricles: the pulmonary artery emerges from the right ventricle, and ramifies through the lungs; the aorta arises from the left ventricle, and branches through every part of the body. Valves are stationed at the roots of the arteries, so as to hinder the return of the blood from the arteries into the ventricles, without impeding the flow from the ventricles into the arteries. Arteries form cones, their apices at the heart, and their bases at their extreme capillaries. An artery is composed of several coats; but two only at present are worthy of our particular attention, namely, the muscular or irritable coat, and the elastic. The elastic is most conspicuous in the large arteries; in the capillaries it is not perceptible. The only termination of arteries which it is necessary to notice for the illustration of our subject, is that in veins by continuity of canal; and the only origin of veins which is requisite to mention, is that from arteries. All the veins ultimately terminate in the auricles, the cavæ returning the blood from the system at large to the right auricle, and the pulmonary veins from the lungs to the left auricle; thus constituting cones, with their bases joined to the extremities of the arteries, and their apices to the auricles. The coats of the veins possess both the irritable and elastic properties. The elastic property, with few exceptions, is not sufficient to preserve the cylindrical shape of the veins when divided, but is capable of restoring them to their original size, after they have been moderately stretched. Many of the veins belonging to the system of the cavæ, especially the superficial ones, are supplied with valves, which allow the current to proceed towards the auricle, but impede its course in the opposite direction.

We shall now go on with the relation of our experimental observations. They were made during my experiments to ascertain the effects of the pressure of the atmosphere when freely admitted into the cavities of the thorax; and also, while inquiring into the cause and the effects of an obstruction of the blood in the lungs. After suffocating an animal by throwing a ligature round the trachea, then removing the sternum, we find the heart performing its function, and the lungs in a state of inflation. During systole the ventricles feel hard, and soft during their diastole. To ascertain the influence of the active power of dilatation of the right ventricle and auricle, the cavæ were compressed, the inferior as near the diaphragm as possible, and the superior a little above its entrance into the auricle. As the auricle was thus limited to the small quantity of blood that the vena azygos and the coronary veins poured into it, it was expected that the blood which remained in the inferior cava, between the compressed part and the auricle, would have been pumped out; but no effect indicating a sudden extraction of blood from the isolated portion of the inferior cava could be perceived. Had the auricle or ventricle exercised the function of active dilatation, it must have been discovered, for the lungs were quiescent; and no muscular action, save that of the heart itself, perplexed the observation. The right auricle accommodates itself astonishingly quickly to a variation in the quantity of the circulating medium; it does not discharge the whole of its contents during its systole. After the last systole of the left ventricle has occurred, irregular and hurried, or fluttering contractions of the muscular fibres of the right ventricle take place, which continue for some time. They are seemingly excited by the stimulus of distention; for the ventricle, from the gorged state of the pulmonary artery, is unable to rid itself of the blood that continues to be forced into it from the cavæ. After the cessation of the irregular muscular action, the right ventricle feels soft, and the left feels contracted. If now one of the pulmonary veins are opened, near their junction with the auricle, no expansion of the latter chamber, or of the ventricle, takes place; but as the residue of the blood in the pulmonary veins is distilled into the ventricle, it imperceptibly softens. If the pericardium is slit open on the cessation of the irregular action of the muscular fibres of the right ventricle, the latter is soon greatly distended. If, presently after the last systole, water at the temperature of 95° is injected into the left ventricle, through an aperture in one of the pulmonary veins, it does not excite it, in my opinion, to a perfect systole, but occasions in it the same sort of fluttering contractions as the irritation of the stimulus of distention induces in the muscular fibres of the right ventricle, as described above. After extracting a heart shortly after the cessation of the action of the left ventricle, and instantly immersing it in warm water, it was immediately thrown into similar agitation to that noticed in the right ventricle from the stimulus of distention. Its action excited considerable motion in the water; but, with every deference to the opinion of others, I cannot say that I perceived the water drawn in, and then, after an interval, thrown out again. The hurried and irregular ejection, or, more properly speaking, commotion in the water, seemed to me to be the effect of the contraction of distinct fasciculi of muscle in different parts of the heart following each other, at first in rapid and irregular succession, and not the effect of the contraction of the whole of the muscular fibres of either ventricle acting in concord as during a perfect systole.

The differences observed in the subsequent actions of the heart and blood-vessels of animals destroyed with the lungs retained in a state of inflation, and those destroyed by collapsing of the lungs, were the following. After the latter mode of destruction, the action of the left ventricle ceased earlier, (reckoning from the instant the trachea was tied, and the instant both the cavities of the chest were freely and permanently exposed to the atmosphere, and the action of the ribs interrupted at the same time.) Also, the impulsive power of the circulation was incapable of impelling the blood so forcibly into the right ventricle, which was evident from the latter offering a less resistance to the touch, and from the more gradual manner it parted with its irritability, after the last systole of the left ventricle. In fact, when the lungs are collapsed, the influence of death alone deprives the ventricle of its irritability; but when the lungs are retained in a state of inflation, the ventricle is exhausted also by the stimulus of distention. When the lungs are collapsed, the branches of the pulmonary artery are compressed, and, from the blood being precluded from entering them, they are comparatively empty; hence, the capillaries and minute veins, especially those on the peritoneum, are finely injected with blood. The aorta is equally empty after the one mode of death, and after the other.

As the physiologists of the present day unanimously agree that the heart possesses the power of active contraction, and that the coats of the arteries evince the property of irritability and elasticity, it is unnecessary to bring forward proofs to corroborate these opinions; therefore, we shall take them for granted, and proceed to the examination of the function of the venous system, whose influence in the economy of the circulation is not so generally understood. When an animal is killed by securing the trachea, the great distention of the right auricle and ventricle, which immediately succeeds the obstruction of the blood through the lungs, must, undoubtedly, be the operation of some other cause, than the active power of dilatation of the cavities themselves. After destroying two animals, one by tying the trachea, so as to keep the lungs in a state of inflation, the other by collapsing them, we observe a difference in the distention of the cavities of the right side of the heart, after the last systole of the left ventricle. The blood is conveyed most vigorously into the right ventricle when the lungs are retained in a state of inflation, which militates against the *vis a tergo* of the arteries being the immediate power which distends it. For a more lucid illustration, we shall suppose the blood to be incompressible, the veins to be passive, and the aortic system through the medium of the blood, to be the impulsive power which distends the right auricle and ventricle. Agreeable to this view, the distention of these cavities ought to be greatest after destroying an animal by collapsing the lungs; for the blood which is admitted into the pulmonary artery, when the lungs are retained in a state of inflation, will be added by their collapsion to the corporeal circulation; therefore, the aortic system will have between it and the right ventricle and auricle, a greater medium or quantum of blood to act upon; hence it ought to be able to distend them more forcibly after collapsion, than when the lungs are retained in a state of inflation. However, according to the appearances noticed in our investigation, the reverse was the case, and the only peculiar phenomenon remarked, as the effect of the impulsive power of the aortic system, after destroying an animal by collapsing the lungs, was an engorgement of some of the capillaries and minute veins. When we take into consideration the position of the veins of the system of the cavæ, which originate from arteries by continuity of canal with respect to the motion of the blood, their forming the base of cones, and their necessarily commencing the impulsive power of that system, (the absorbent branches of the cavæ give their contents the first impetus, and the pulmonary veins do the same in my opinion), we must, I think, agree with the celebrated Mr John Hunter, in considering the strength of veins to be in an inverse proportion to their size. This conclusion offers an explanation of the cause of the greater distention of the right ventricle, when the lungs are retained in a state of inflation, than when the animal is destroyed by collapsing them. When the lungs are retained in a state of inflation, the whole of the circulating fluid which is propelled from the system of the aorta, into that of the cavæ, is easily accommodated in the system of pulmonary artery, the right cavities of the heart, and the larger branch of the cavæ; so that the capillaries and minute veins have no impediment to encounter, in forwarding the blood which they receive from the aortic system, towards the right cavities of the heart. On the contrary, when the lungs are collapsed, a considerable reservoir for the blood which is impelled from the system of the great aorta is lost, by the closing up of the system of the pulmonary artery; and the larger branches of the cavæ and the right cavities of the heart are not sufficiently capacious, independently of that reservoir, to contain it easily; therefore the capillaries and minute veins are early impeded in forwarding their contents, by the stagnation which takes place in the larger veins. From this cause, the minute veins are distended by the blood which continues to be propelled from the aortic system, and, of course, their impulsive power is consequently greatly diminished. Thus, from the privation of its main strength, the system of the cavæ is unable to propel the blood so vigorously into the right auricle and ventricle, when the animal is destroyed by collapsing the lungs, as when the latter are retained in a state of inflation.

We shall next take into consideration the active power of dilatation attributed to the heart; and, in the first place, we shall inquire into its nature; then compare its characteristic qualities with the phenomena already described; afterwards we shall be able to judge whether we have arrived at any facts capable of furthering our acquaintance with the moving powers of the circulation. As Dr Wilson Philip is the last author on our subject, whose writings I have perused, and which has already been alluded to, I shall take the liberty of quoting the following passages from his Essay, so as to give my readers a correct statement of the ideas entertained by Dr Philip as to the nature, and the influence exercised by the inherent dilating power of the heart, and also the resilience of the lungs. "What purpose then" says Dr Philip, "is served by the dilating power of the ventricles, increased by the tendency of the lungs to collapse? It favours the entrance of the blood suddenly propelled into it by the contraction of the auricle; and the degree of dilating power is well proportioned to this office. Without this dilating power, the tendency of the ventricle would be to remain in a state of collapse after the systole, and part of the power of the auricle would be expended in dilating the ventricle. Here, as in many other instances, both in man and the inferior animals, we see nature saving the muscular by the substitution of the elastic power." In the last sentence, we perceive Dr Philip recognising elasticity to be the nature of the inherent power which enables the ventricles to dilate themselves. Pray what is the action of an elastic body? Mr John Hunter thus defines it, "The action of elasticity is continual, and its immediate effects are produced whenever the resistance is removed; by which it may be distinguished from other powers." * From our definition we learn, that the action of an elastic body is permanent, and that, as soon as the resisting power which retains it in a forced position is removed, it immediately regains its natural state of rest. In order to elucidate our problem, we shall admit the body of the left ventricle of the heart to be possessed of an elastic property. As the systole of the ventricle throws the elastic property into a forced position, and as the ventricle remains for some time after its last systole in a comparative state of collapse, we have only to do away with the influence of the power which retains it during that period in that state, and the elastic property will instantly restore itself to its natural position. Before we set about releasing it from its constrained situation, we shall inquire into the nature of the power which we have to contend with. As a state of relaxation in a muscular fibre succeeds the state of contraction, it follows, that the action of the muscular fibres of the walls of the ventricle is not the cause of the elastic property being retained in its unnatural position, for we admit contraction to be the last motion of the ventricle. The resisting power, then, must arise from the propulsion of a portion of the blood from the cavity of the ventricle, by its systole, into

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* On the blood.

the aorta, without the ventricle being able, by its elasticity or active power of dilatation, to draw its wonted supply in return from the auricle. Therefore, as the elastic property endeavours, by its reaction, to regain its natural state of rest, a tendency to form a vacuum in the cavity of the ventricle must be the result, which effectually retains the elastic property in its constrained position. Now, if we can establish a communication between the cavity of the ventricle and the exterior air, it is evident that we shall be able to do away with the tendency to a vacuum, and consequently with the resistance offered to the reaction of the elastic property. Such a communication is easily established (without doing any injury to the walls of the ventricle), by opening one of the pulmonary veins near its junction with the auricle. In the narration of our experimental investigation, we are informed, that after such an expedient was had recourse to, no such phenomenon as the dilatation of the ventricle was remarked. Therefore, if we can depend on the correctness of the observations during the above experiments, and if observations under such circumstances can be relied on, we must conclude, the active power of dilatation, or the action of the elastic property of the auricles and ventricles, to be either ideal, or to be so extremely feeble, as to be capable of evading our senses under the most favourable situation in which we can place the organs for inspection; for I can scarcely believe it possible to devise more decisive modes of ascertaining the existence of such a power, though ever so trifling, than those to which we had recourse.

Further, as the action of an elastic substance is as perfect after the extinction of life, until the process of putrefaction destroys its texture, as during the life of the animal; by examining a detached heart at a butcher's stall, after its absolute death, or after its utmost contraction by the *vis mortua*, we can readily satisfy ourselves whether the walls of the ventricles have any elastic property that can be appreciated. If we find a heart contracted, and, on pressing its body so as to flatten it, that it does not present to our senses a disposition to recover its natural shape, similar to what we witness in the truly elastic arteries whose roots are attached to its base, what inference are we to draw? Why, certainly, we must infer, that it possesses no greater elastic property than muscles in common. The condition of the heart greatly depends on the state of the animal when killed. Fat beasts, from their unwieldiness, and from the action of their diaphragms being restrained by their obesity, are easily overdriven, and sometimes, on their way to the slaughterhouse, to prevent their suffocating, they are obliged to be *stuck*, or, from urgency, they are killed while yet breathless and ready to faint. The right ventricle of the heart of an animal killed in such a plight, is found to be gorged; and the reason appears to me to be obvious. As the blood is more or less obstructed in its passage through the lungs, previous to the sticking of the animal, the pulmonary artery, and the cavities of the right side of the heart, are necessarily more or less gorged, and the ventricle and pulmonary artery must remain so; for, during the time the animal is bleeding to death, a small portion only of the blood which they contain at the time they are stabbed can pass into the system of the pulmonary veins, for want of pure air to enable it to undergo the necessary change in the *rete Malpighii*. Thus, the right ventricle will be large and flabby; for, in consequence of its being retained in a state of extension, the action of the *vis mortua* is prevented from affecting its muscular fibres. When an animal is killed by dividing the large blood-vessels of the neck, without any previous obstruction in the lungs, then no engorgement can take place in the right ventricle; for the blood rushes with unusual impetuosity towards the point where there is the least resistance, and, in a few minutes, nearly the whole of the blood in the body escapes through the artificial outlets. I think the gorged state of the right ventricle of the heart of an animal, when killed while breathless and faint, to be another evidence in support of my opinion, that prostration of strength, arising from short continuance of anxious exertion, is the immediate effect of an obstruction of blood in the lungs. * In an animal that dies a natural death (without losing shortly antecedently much blood), the distention of the right ventricle bears a comparative relation to the vigour of the blood-vessels on the obstruction of the blood in the lungs; and the influence of the *vis mortua* affects the right ventricle in an inverse ratio to the extension of its fibres. I presume, that it was the flabby appearance of the right ventricle, arising from the above cause, that induced Mr John Hunter to think the heart less susceptible of being affected 'by the stimulus of death,' than the other muscles of the body.

Dr Wilson Philip also considers, in our quotation, the dilating power of the ventricles to be increased by the tendency of the lungs to collapse. The following negative evidence will, perhaps, cast some light on this obscure point. First, the ready tendency of a lung to resume the state of inflation, after reducing it to a comparative state of collapse, promises no aid to the power which dilates the ventricles.† Again, the function of the heart is capable of being carried on, for the space of half an hour, independently of any assistance from the resilient power of the lungs, as exemplified by the admission of air into both cavities of the chest of an animal simultaneously. Here the tendency to form a vacuum was overcome, consequently the influence of the resiliency of the lungs was abolished. The latter negative evidence proves, that the dilatation of the ventricles can be effected without the assistance of the resilient power of the lungs. The assumption, then, of the tendency of the lungs to collapse, aiding the dilatation of the ventricles, is, in my opinion, interposing an unnecessary link in the chain of causation.

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* See Medical and Surgical Journal, LXXVII, p. 535.

In rational inquiries, when it is impracticable to adduce unequivocal evidence, we should doubtless incline to the side of the question which appears to be most consistent with known facts. For this reason, I am induced to consider *the body* of the heart to be a hollow muscle, having as little intermixture of elastic substance blended with its fibres as any muscle in the animal frame; and to think with Mr John Hunter, that 'the contracted state of a muscle, as well as the relaxed, arises from a power inherent in itself; but the recovery, or elongation, must depend on some other power;' therefore, 'an elongator of some kind or other is required to enable every muscle to renew its action.'‡ To explain the mode in which I think the dilatation of the right ventricle to be effected, we shall first of all examine the condition of the cavæ, and of the right auricle and ventricle, at the termination of the systole of the latter. At this instant the cavæ appear full, and the auricle has been distended to the acme of its diastole by the cavæ through the medium of the blood, which is its elongator; and its muscular fibres are so far recovered since their last action, as to be in a state ready for receiving the impression of the stimulus which is to excite them to systole. The ventricle, as soon as it arrived at its utmost state of systole, became perfectly relaxed and inactive, with still a portion of blood in its cavity; so that it is placed under the most favourable condition for the extension of its muscular fibres, by the blood which is to be propelled into it from the auricle. The auricle contracts, and, by its systole, it dilates the ventricle. During its elongation, the ventricle remains passive. When the auricle contracts, the blood in the cavæ offers a sufficient resistance, to prevent the expelled fluid taking a retrograde course. At the conclusion of the systole of the auricle, the condition of the two chambers will be reversed; the ventricle will be in a state of diastole, and the auricle in that of systole.

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† Medical and Surgical Journal, LXXVI, 553.

‡ On the Blood.

The foregoing disquisition appears to me to favour the following exposition of the circulation. We shall commence with the left ventricle of the heart in a state of diastole. By its systole, a quantity of blood is thrown into the system of the great aorta, which communicates an impulse to the blood already contained in it. The momentum given to the blood by the first impetus, is partially expended on the dilatation of the vessels; for the augmentation increases their diameter, though inappreciable by our senses. The vessels being elastic as well as irritable, recoil on the blood, by which they were distended, and first of all their trunk, from its sustaining the more immediate impetus of the ventricle. By this reaction, the blood receives a fresh impulse. As we trace the arteries towards their extremities, their elastic property gradually decreases, while the irritable increases, and also its influence as an impulsive agent. At length we lose sight of the effect so characteristic of the action of the first impetus, and we are no longer able to detect the elastic property in the coats of the vessels. We now find, in a very considerable portion of the greater circulation, the irritability of the vessels alone adequate to the impelling of the blood along their channels. Thus, by the combined influence of the left ventricle, and of the elasticity and irritability of the aortic and cavæ systems, the blood is conveyed from the left side of the heart, through the greater circulation, to the right side, with sufficient momentum, when it arrives at the right auricle to distend it when in a state of relaxation, and, during its systole, to act as a buttress to its contents, so that it may direct its whole power against the relaxed ventricle, which it is its office to replenish. The right ventricle gives the first impulse to the blood in the less circulation, in the same manner as the left ventricle does to the greater circulation; and the passage of the blood through the two circulations, and also the dilatation of the right and left chambers of the heart, are effected by similar powers.

III.

Cases of Neuralgia of the Face, and painful affections of the Limbs, successfully treated. By Robert Evans, M. D. Member of the Royal College of Surgeons of London.

In stating the following Cases, I have no object in view but to contribute my mite of experience to the general stock of practical information; and, as Neuralgia seems to have yielded to various modes of treatment in the hands of different practitioners, some tending to invalidate the late recommendation of iron, and others to strengthen it, I conceive it to be my duty to bear testimony to the efficacy of Mr Hutchinson's mode of cure, by publishing the two following Cases, which were completely cured by the carbonate of iron, after other means had failed.

Case.—J. B. Esq., aged about 50, of regular and most abstemious habits, and otherwise of a good constitution. In early life, from exposure to cold and wet, and sleeping in a damp bed, he contracted an asthmatic disposition, but which has left him for many years. He had been for years troubled with a pulsating sensation in the base of the brain, which was ascribed to either an aneurismal affection there, or organic lesion, and still continues without affecting his health. His present disease was seated in the branches or twigs of the ophthalmic branch of the fifth pair of nerves of the right side. It occupied at first a circular spot the size of a dollar, and about 2½ inches above the supraorbital foramen, and near the anterior origin of the temporal muscle. There was no pain in the trunk, as it arises from the foramen, or for 2½ inches above it. The pain was not constant during the day, but was always excited by the most trivial touch of the finger, or any external agent. The force of a slight current of air striking on the face would cause it. The sensation of pain was like creeping or dragging; and as if there were many threads closely distributed from a central point to half a circle, and by pulling the centre stalk or trunk, you suddenly dragged the distributed threads. Every motion of the jaws, as in eating, excited it; but never any force of pressing the jaws otherwise together. Speaking would cause it. The paroxysm would come on in walking, and so severely as to require him to stand still for some minutes, when it would go off, but it was never excited by riding. The weight of the water falling from a shower-bath which he was using, at times would excite it. When the disease had advanced more, the paroxysms were so severe that he could not take solid food, owing to the pain being excited by chewing, and he was compelled to subsist on panada, soups, and thin spoon-meat, &c.; and when the fit would be at its height, it would show itself in a twig of the superior maxillary nerve, darting down to and into the eye-tooth, distorting the angle of the mouth. When once quiet in bed, he would scarcely ever feel any thing of it for the night. This state of the complaint was present for upwards of eighteen months, when regular treatment was applied.

Narcotics were ordered; and opium seemed the most effectual, but only in suspending the pain for a time. As Mr Abernethy had been successful in curing this disease through the digestive organs, * I gave a fair trial to his mode of treatment, but without any benefit.

Cinchona rubra, joined with an aromatic, was given for several days as largely as his stomach could bear; and though successful with other practitioners, it was of little benefit here. Under the use of the bark, the disease changed its seat from the original part to the right temple, into the hollow behind the outer angle of the eye. In this new seat, it was excited by every motion of the mouth and eyelids, as in winking. Six leeches were applied to the part and its vicinity, which bled well, and with the effect of removing it from this place; but it then became most severe in the trunk of the branch as it leaves the superorbital foramen, for two inches. He now suffered no pain from the time of lying down in bed at night, till dressing the next morning. Leeches were applied to the part and bled well, but now only relieved the pain for a short time; and when it returned, it was severe in a twig running round the margin of the orbit to the external canthus, and excited by touching the eyelids, or even the eyelashes. Cloths wetted in a cold saturnine lotion relieved the pain for a time. After some short time, the pain returned to the part originally affected, severely, and more constantly, with intervals of ease of only 15 minutes. In this advanced stage, light seemed to make an impression on the retina, giving a sense of fulness in the globe of both eyes, but greater in the diseased side. Any sudden intelligence, or emotion of mind, would now induce the attack, whether in or out of bed, showing the connexion between it and the mind.

Various narcotic medicines were used locally in the form of plaster over the part, and internally, but without much benefit. Narcotics would have been pushed further, but witnessing their fair trial, and failure, in cases which occurred in the Royal Infirmary in Edinburgh in 1820, and from information obtained from a valuable practical Thesis on the subject by my fellow graduate, Dr R. M. Kerrison, † I felt it necessary to discontinue their use. Tincture of hyoscyamus was used without effect, though it cured cases in the hands of Dr Home, as mentioned in his Clinical Lectures. Arsenic was not given. Belladonna was not used, as it failed with other cases.

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* Surgical Observations on Constitutional Origin, &c. of Local Diseases, 1809, page 104.

† See Tentamen Medicum inaugurale de Neuralgia Spasmodica Faciale. By R. M. Kerrison. Edin. 1820.

From Mr Hutchinson's commendation of carbonate of iron, and witnessing its benefit in a case of neuralgic affection of the sciatic nerve of the right side, in the summer of 1820, in the Royal Infirmary, under Dr Home, it was agreed to give it a trial. He accordingly took ʒj. three times in the day, mixed up in panada, gruel, &c.; and in a few days, I was surprised to find his disease nearly cured. The medicine was continued for some time longer, attending to the state of his bowels, when he became perfectly cured, and has remained so ever since, now upwards of two years. There was some reluctance in giving this medicine, owing to the pulsation in the head, till another physician acquiesced in a trial.

Case of S. M'Ilwee, aged 45, affected with neuralgia, or tic douloureux, of the superior maxillary nerve, affecting all the teeth supplied by it, and the side of the nose, but not extending to the *portio dura.* It willbe unnecessary to occupy more time in giving a minute detail of this case than to state that her sufferings were most distressing. She had been affected with this dreadful disease for the space of three years, during which time she had nearly all the teeth of the upper jaw of that side extracted, without relief. The disease continued in paroxysms with very short intervals of ease. It became so insufferable, that she, in general, was a total stranger to sleep. She could neither chew nor eat solid food, but took her little nourishment in a fluid form. She was affected by the most trivial causes, as in Mr B's. case. When she applied to me, she seemed like a person deranged in her intellect, from pain. I put her on a course of *carb. ferr. precip.* ʒss. three times in the day, guarding against constipation; and in between two and three weeks she was perfectly cured.

Three cases have occurred, two in the arm and one in the leg, with symptoms resembling neuralgia, which, from their importance, I am desirous of inserting here, although, in the end, they proved to depend on a morbid and inflamed state of the parts, at or near the vertebra, where the nerves of the corresponding members originate. The first case was treated for a short time as neuralgia, till the patient complained of pain and uneasiness in the back, which led to the knowledge of the true seat of the disease.

Case.—The Rev. S. B. aged about 45, of a strong and robust habit of body, complains of what seems to be a neuralgic affection of the nerve, in the ham of the left leg. He is unable to ride, sit, or walk, from the excruciating pain he experiences between the hamstrings and through the calf of the leg. The pain is constant, but comes on more severely in paroxysms, darting upwards as quick as lightning. The leg is swelled, and its calf red and painful to the touch, but partly from friction with rubefacients. He has no ease in any position, though change of position for the time gives him relief. He feels as bad in bed as out of it. He gets no sleep, and has lost his flesh and strength. Appetite is greatly impaired. The alvine dejections are fetid, black, and scanty. The pain is aggravated by jolting, or striking the foot against a stone or other projection. He can cover the extent of the pain in the ham with the palm of the hand. Latterly, there is a numbness in the inside of the calf of the leg. He mentioned no other pain or uneasiness, or appearance of disease about the leg or thigh. The treatment consisted of leeches, fomentations, cold saturnine lotions, rest, purgatives of calomel and rhubarb, repeated often, and keeping the bowels regular in the intervals, under which he seemed a little relieved; but, not improving so much as was expected, he was put on a course of the *carb. ferri precip.* While using this medicine, he fancied he improved a little. He now, for the first time, mentioned a pain he felt some time ago in two of the lumbar vertebræ, which he ascribed to his sleeping in a damp bed, and riding previously in rain, and before he perceived any thing wrong in his leg. This was verified on examination. There was no redness or swelling over these vertebræ, but pain was excited in them by pressure with the points of the fingers, which caused the pain to fly down from these joints, in the course of the sciatic nerve, to the pained part of the ham and leg, aggravating in a great degree the affection there. After repeating the examination, and the patient himself being satisfied of the connexion of the affection of the leg, &c. with the spine, we stopt the further use of the carbonate. He was averse to leeching or blistering the spine, but consented to the extensive use of ointment of tartarized antimony to the vicinity of the diseased spine. He continued to attend to his bowels; and in a few weeks, continuing the use of the ointment as much as he could bear, unassisted by any other remedies, the affection of the ham and leg began to decline and get well. While writing this, he is free from pain or uneasiness, and only a slight numbness remains. He has regained his sleep, flesh, and strength.

Here, it is evident, the disease of the leg was sympathetic, from either an inflamed state of the investing membranes, or effusion under the neurilema of the sciatic nerve at its origin, and the affection of the leg depended on that state, as pain and numbness of a remote part of the body are often owing to pressure on, or some morbid state, of some part of the brain, as precedes some apoplectic attacks. The cure of the case proved it. The result of the foregoing case clearly laid down the treatment of the following two cases.

Case.—Miss W. T., aged about 40, never married, of a thin spare habit of body, a hereditary strumous constitution, after exposure to cold, as she thinks, complained of a most severe and excruciating pain all over the left arm, hand, and shoulder, at times only in the tendinous structure about the wrist, and there of an aching kind; the pain darting down the course of the nerves to the finger ends, with numbness of the fingers. She was unable to suffer the hand or arm to remain one minute in one position. The pain was continued; pulse not affected; appetite a little impaired. She was subject to periodical headachs. There was no external visible morbid appearance in the pained parts. She complained of a great sense of heat flashing through and along the arm, to the hand and ends of the fingers, with some severe twitches of pain through the *latissimus dorsi* and pectoral muscles, though there was no thermometrical increase of temperature. On examination, there was found a blush of redness over the first dorsal vertebra, and considerable pain in that place on pressing with the ends of the fingers, the pain shooting down from this joint with increased force and violence, through all the arm and hand, while no pain was perceived in the other joints, either above or below, nor was the affection of the arm increased or aggravated by pressing on the other joints. She suffered several smart shocks of pain in the *right* arm, proceeding from this joint, with numbness of the finger ends, but transitory. The pain of the left arm shifts from the wrist to the elbow-joint, and to the shoulder. Every muscular exertion increased the affection of the part. Her nights were sleepless, and yet no feverish state was present.

In the treatment, local abstraction of blood from the pained joint, by cupping-glasses and scarificator; and, leeches often repeated, to the spine; with blisters, gentle aperient medicines, combined with small doses of calomel occasionally; narcotics internally, as extract of stramonium and lactucarium, hyoscyamus, conium, &c. But no article of that class gave so much relief as opium and tinct. opii, guarding against its effects on the bowels. Narcotics were applied to the arm and hand, but without much benefit, though she thought otherwise. A singular sensation, more clearly manifesting the nature of the disease, was experienced, while applying a sponge wrung out of hot water to the bite of the leeches over the inflamed joint; the heat was not at all perceived or felt at the joint, but in that part of the arm at the time suffering pain, as the elbow, &c.; and the heat was felt sensibly passing down from the joint to the part affected. Surgeon-general Crampton of Dublin was consulted, and acquiesced in the nature and treatment of the disease. The sympathetic affection of the nerves, however, and of the tendinous structure, not getting quite well, though much relieved, the remedies were left off, under the impression, from fresh advice, that the disease was tic douloureux; and narcotics and bark, camphor, colchicum, mercurial ointment, were used in succession for the arm; and, after all, belladonna, in the form of fomentation and poultice, with shamoy leather to envelope the arm, are said to have effected a cure, though not quite perfect. Now it is evident, the affection of the arm and hand was either sympathetic of an inflamed condition of the involucra of the spinal marrow issuing to form the nerves of the member, or of the nervous substance itself, which was so far removed by the previous treatment, or there was some degree of thickening of, or morbid deposition in, the neurilema, or nervous substance, from the preceding local excitement, or effusion of organizable lymph, impeding the functions of these nerves. The affection of the right arm, though not of long duration, corroborates this view. The disease, therefore, was clearly depending on a diseased condition of, or about the origin of, the nerves of the arm; and the following case leads to the same conclusion.

CASE. JANE CLARKE, aged 20, of a thin and delicate habit of body, complained of a severe excruciating pain of the left arm and hand, with numbness. She had been ill some time before applying for advice, and had even lost the power of, and feeling in, the thumb. It seemed, from its long continuance, to have arrived at a chronic state. In short, she was precisely, in detail, affected as Miss W. T. was. She complained of a severe painful state of the first and second dorsal vertebræ. Pressure on these joints with the fingers, or a hot dry sponge, caused a simultaneous increase or aggravation of the affection of the arm and hand, but not in the thumb, which was paralytic. There was numbness in the ends of the fingers. All her sufferings were referred to the hand and arm, as with Miss W. T., paying no attention to the state of the spine, till examined.

In the treatment, local applications to the spine, and aperient medicines, were used; but the most effectual and permanent relief was obtained from the continued application of ointment of tartrite of antimony to the joints affected, and their vicinity. Whenever the pustules, from the ointment, began to fade, she renewed it, and continued this plan for many weeks, and is still going on with its use, though now, I may say, almost perfectly well, except the thumb, which in all probability will never be restored to its natural and healthy state. During the above treatment, nothing was applied to the hand or arm. I will not occupy the space of the Journal in making any further observations, as the nature and seat of the disease must be sufficiently obvious from the cases stated.

Magherafelt, Ireland, *Oct.* 24. 1823.

IV.

Case of Tic Douloureux, successfully treated by Carbonate of Iron. Communicated by GEORGE A. BORTHWICK, M.D. F.R.S.E. Fellow of the Royal College of Physicians, Edinburgh.

G. N. Esq., æt. 46, of a corpulent habit and sedentary life, complained of a rheumatic affection in the right hip in December last, which was of short duration. He was very soon after seized with a severe pain in the whole of the left side of his face, which continued for a few minutes; and, as the paroxysm subsided, the pain generally fixed in some particular tooth. Throughout the months of January and February, this painful affection continued to recur at intervals, and gradually went off as the weather became milder. Mr N. continued well until the middle of June, when the pain again attacked him in the same cheek with great severity. Under the direction of Mr N.'s medical attendant in the country, various local remedies were tried, without any sensible relief. In the beginning of July, some diseased teeth were extracted; and it was observed that the paroxysms were afterwards more acutely felt in the sockets from which the teeth had been pulled. About this time, in consequence of a febrile state of the pulse, venesection was twice performed, by which the restlessness and general uneasiness were relieved. The paroxysms, however, continuing with unabated violence, Mr N. came to Edinburgh, and put himself under my care on the 14th July.

On this day, the disease appeared in great severity; while stating his case to me, he had several paroxysms, some at the interval of three minutes; the approach of the attack was felt for a few seconds before it was in force; during its violence, the muscles of the face, particularly of the eyelids and mouth, were much convulsed. Saliva flowed profusely, and the patient stamped about the room in extreme agony. The fit was somewhat lessened in intensity by rubbing the affected cheek strongly with the hand; when this was not done, the attack subsided more slowly, and degenerated into a tedious aching pain. I observed that speaking caused a frequent recurrence of the paroxysm; and Mr N. assured me that, within the last few days, any attempt at mastication, or swallowing liquids, or the accidental touching of the cheek, nose, or lip, invariably caused an attack of the pain. The duration of the paroxysm was about fifteen seconds, and its termination very abrupt; for while it seemed at its height, and the patient in the utmost agony, he removed his hand from his face, looked around him calmly, and appeared surprised at the sudden cessation of his torture. The pulse, during the remissions, was 60, but was raised to 75 and 80 by an attack. The skin was cool; the bowels regular; the appetite good; the pains were most violent about two hours after dinner, and during the night.

As the disease appeared to be a well-marked and severe case of tic douloureux, it was resolved, in consultation with my friend Dr George Wood, to give a fair trial to the carbonate of iron, a remedy so highly extolled of late in similar cases; and our patient was ordered to commence with half-drachm doses thrice daily, while at the same time some very gentle medicine was given to keep the bowels easy.

On the 15th, three doses of the carbonate were taken; some severe paroxysms during the night; but some sleep was obtained, probably owing to a few drops of Battley's sedative liquor having been taken.

16th.—Three doses of the carbonate taken to-day; has had very few attacks of pain; and instead of the pain commencing, as hitherto, with a tremulous motion in the left eyelids and cheek, and a forewarning of some seconds, it came on suddenly in the cheek and jaws, parts in which it had usually terminated. Mr N. this day wrote three letters; an exertion which he had not been able to make for the last fortnight without an immediate accession of pain;—he ate some dinner; conversed more easily; can touch the affected cheek with impunity, and feels, as he says, "quite a new creature," having had only seven paroxysms since morning. To continue his medicine.

17th.—Slept better; in the morning had two pretty severe attacks; the carbonate to be continued in drachm doses.

18th.—During the last twenty-four hours has had only two paroxysms, one at six o'clock this morning, and another while putting on his clothes. Is allowed to take an airing in a carriage this forenoon; and the dose of the carbonate is increased to a drachm and half thrice daily.

19th.—Is much better: felt refreshed by his excursion yesterday, and slept well at night; had no return of his complaint until breakfast time this morning, when he had two slight "threatenings" which immediately subsided on rubbing his cheek with his hand. To continue his medicine.

20th.—Slept well; had two slight attacks yesterday evening after dinner, which were of very short duration. This morning Mr N. showed the affected cheek with impunity, and, for the first time since his illness, he ventured to use his tooth-brush without any painful effects.

21st.—Felt so well yesterday as to go twice to church without inconvenience; had three twitches of pain between four and six o'clock this morning, which were merely instantaneous.

22d and 23d were passed comfortably; each day had two very slight pains.

24th.—No return of pain during twenty-four hours; shaved this morning with perfect ease, and now moves his jaws fearlessly while at his meals. To continue the medicine.

26th.—Has continued free from complaint; returns to the country this day, with directions to continue the carbonate for some days.

I saw my patient during the autumn in the country, and found him in perfect health; and, in a letter which I received from him, dated 5th of this month, he informs me that he continues quite well.

It is not my intention to comment upon the foregoing case: Those practitioners who have treated this formidable, and sometimes hopeless, disease, will best appreciate the severity of the case now recorded. I communicate it to the profession as an additional and satisfactory instance of complete success obtained by the use of the carbonate of iron, a remedy which has of late been so beneficial in the hands of Mr Hutchinson in similar cases.

83, *George Street, Edinburgh,*
15th November, 1823.

V.

On the Dose of the Pulvis Antimonialis. By F. HAWKINS, M.D. Extraordinary Member of the Royal Medical Society of Edinburgh.

To ascertain the precise doses, as well as the medical properties of those medicines which we are in the daily habit of prescribing in the most painful and dangerous diseases to which the human frame is liable, is an object of great interest and importance to physicians, and one which is certainly essential, in order to ensure either safety or success to their practice. Of the many valuable and powerful medicines whose virtues have been long known and duly appreciated, the subject of the present communication has been usually esteemed not the least efficacious; but, if I mistake not, the facts about to be related will show, not merely that its merits have been rated too highly, but that it is (in ordinary doses) *entirely inert*, and, as a medical agent, altogether useless. Every person, whether in or out of the profession, is well aware how generally the *Pulvis antimonialis* is employed. This medicine was introduced into the Pharmacopœia in imitation of, and as a substitute for, the celebrated nostrum of Dr James, which bears his name. The *pulvis antimonialis* of the London, the *oxydum antimonii cum phosphate calcis* of the Edinburgh Pharmacopœia, is usually exhibited in doses of iv. v. or vi. grs., with the intention of promoting diaphoresis; and if the dose be augmented by a few grains, we are told that nausea and vomiting, and sometimes purging, will be the consequence. But that no such effects *can be produced* by such an employment of this medicine, I am quite convinced, from having repeatedly observed, that no effects of any kind have followed the exhibition of the medicine, in doses of ten times the quantity. In those affections of the head which seem to arise from an irregular determination of blood, I have several times employed it; at first in the manner, and with the caution, recommended in the first volume of the Dublin Hospital Reports by Dr Cheyne, by whom, I believe, this medicine was first prescribed in those complicated and unmanageable diseases which depend on local congestion in the encephalon. The patient began the *antimonial* course with the dose of gr. iv., which was increased gr. ss. every night, till it amounted to ℈i. Finding no sensible effect produced by what I then considered an enormous dose, I proceeded by adding gr. i. every night, till ʒss. was taken; still no effect; gr. v. were added, till the dose reached ʒi., which was given night and morning, for many weeks, to a young lady affected with vertigo and epilepsy, and certainly with some degree of benefit. At last, she has improved under the use of medicine; and she herself attributes her amendment to the powders; but when asked what effect they produce, she replies, " none that I perceive, excepting that they make me feel well. " Beyond this dose I have not proceeded, not that I by any means suspect that a larger dose would be followed by more decided effects, but the bulk of such a dose is extremely inconvenient for the patient to swallow.

To another patient, similarly affected, ℈ij. were given twice a day for some weeks; no function was in any degree affected by it; he was not relieved. A servant of my own took ℈i. every night for a week, after which she left my house, and the medicine was discontinued. Other patients have taken the medicine in equally large doses, and with the same result. It never produced the least inconvenience (excepting from the bulk of the powder) to any of them. I have lately relinquished the administration of this medicine in the minute doses commonly prescribed, and now begin the use of it, by exhibiting ʒss. at a dose. In what I have said it is by no means my wish to rob the family medicine-chest of one of its most important remedies; for such a situation it possesses a qualification of no common order,—I mean innocence. But I do conjure physicians not to rely on a medicine which (administered in the usual manner) is wholly inefficacious; supposing it even to possess all the good qualities ascribed to it, it will still be in some degree a superfluous article of the Materia Medica, as we have so excellent a substitute in the tartrite of antimony, whose medical properties no one can doubt. What has been said may stagger the faith of those who read this letter, as it has done that of those to whom the substance of it has been related in conversation. Let it be received with just so much scepticism as shall induce every physician to put what I have said to the test of experiment; let them begin in the cautious manner recommended by Dr Cheyne, and by continuing the use of the medicine, they will find that what may be called these enormous doses may be given with perfect safety. We sometimes give the appellation of "opprobria medicorum" to diseases, for which we are without either a palliative or cure, but, as I think, unjustly; but must it not be considered as an opprobrium, that we should be in the daily and hourly habit of prescribing a remedy, whose doses and effects we are entirely ignorant of? How little can we depend on authority? Every circumstance of life teaches us the lesson—*Nullius jurare in verba.*

To obviate any objection that may be made on the supposition, that medicine was either adulterated or badly prepared; it may be stated, that a quantity was procured from the Apothecaries Hall, but which, in its effects, precisely agreed with that in ordinary use.

Hitchin, 20*th*
May, 1823.

VI.

On the Effect of External Pressure on the Perineum, in preventing its Laceration during Labour. By Edward Thompson, Whitehaven, Member of the Royal College of Surgeons, London.

When the os externum is opened slowly and at intervals by the child's head, the parts are seldom inflamed or lacerated.—Smellie.

It is an object to be desired in the conducting of a labour, to save the perineum from being lacerated or rent. The usual mode of effecting this is, to give support during the time the head is passing the outlet; and it is said, by those who rest all their hope on such counterpressure, that a rent will be the consequence, if this precaution be not taken. It has not however been satisfactorily proved, whether the support thus given operates solely on the stretched perineum, giving it strength to resist the pressure of the fœtus, or by keeping back the advance of the head, and by so doing, assisting the part to relax and recede. The instruction given is, that counter-pressure must be applied to the particular spot, in the greatest state of distention. If the part be of an irritable structure, as writers allow sometimes to be the case, it is possible that the force used may facilitate what it is meant to prevent; and the perineum, instead of being saved, be eventually torn. A substance, soft in comparison, is placed between two bodies, strongly opposed to each other; if there be a disposition in any part to give way, it is the softest body, and the perineum, being such, suffers. Hence we hear frequently of laceration taking place, notwithstanding the perineum had been jammed between the head and hand for hours, to prevent it. This either shows that the pressure had been wrongly applied, or that the method pursued is in itself defective. In using pressure, so little art is required, that I cannot imagine a person could get wrong, if his intention was to support the perineum at all. The defect of the plan must be allowed, then, if pressure does not succeed in preventing laceration, and every man's practice will furnish him with instances where it has failed.

The method pursued and recommended by Mr Barlow, a gentleman whose attainments have been fully acknowledged, is to attend principally to the slow exit of the head,—it is kept back by the fingers till the perineum becomes relaxed, and drawn back to the farthest. When this is effected, the head may be allowed to escape without fear of laceration, making support by the hand a trivial or secondary consideration. Ever since reading his observations, I have followed this plan, particularly in first children, with decided success, not the slightest rupture taking place in any part of the perineum. The hand acts, when used in this way, more on the principle of an inclined plane, than as intended for a support to the soft parts.

Dr Campbell, in his excellent observations, which appeared in the Journal, No. 77, does not allude to this mode. This is rather surprising, as he cannot but look upon it as advantageous in many cases. He may have viewed it as only a modification of the old practice; but the difference is great, counter-pressure being applied to the perineum not merely on the principle of saving it from rupture. Supporting the perineum strongly with the hand, has frequently a power of exciting the forcible action of the uterus, and in this way hurrying the labour before the parts can have time to relax. Such irritation must be hurtful, particularly where the parts have been before ruptured. The uterus is in a manner made to exert itself, before other parts are prepared to admit the exertions with safety. A broad and thick perineum is pressed forward, and if the hand save it from being torn, it is more than we have any right to expect. I can readily imagine, that the absence of these extrinsic circumstances may be a cause of preventing laceration from taking place, where labours are effected without help, and in secret. The uterus in these instances acts naturally, and the soft parts as naturally dilate, one set of changes succeeding the other in an uniform manner. This, and the many cases we are called to, where the labour is finished before our arrival, without any accident of the kind occurring, would seem to say our interference, by the way of support to the perineum, is often productive of harm, especially if, by it, the delivery be prematurely hastened.

The plan of Mr Barlow will be found in every instance superior to the one commonly employed, as not only does it prevent laceration of the perineum, but, if rightly performed, of the frœnum also. So much cannot be said for the one alluded to, as, in primary labours, it frequently fails in this respect, which is sufficient to direct the attention to another method, if that promise greater success. It was the practice of old, to draw the perineum over the head with the fingers, and I cannot find, that where the parts were fully relaxed, this was followed by rupture. This speaks in favour of what this paper recommends, that it is by attention to the full retraction of the perineum, that a laceration of the part is to be prevented.

Whitehaven, October 18.

VII.

Case of Tetanus from Laceration of the Median Nerve. By ROBERT LISTON, Surgeon, and Lecturer on Surgery, Edinburgh.

WILLIAM BOYLE, æt. 14, in leaping off the back of a carriage, had his hand a good deal lacerated on one of the spikes. He was dragged a considerable way, till some person passing relieved him from his painful situation. The wound was dressed by an apothecary, and some 4 or 5 days after the accident, he applied for advice on account of the consequent pain and swelling. There was then but little suppuration from the wound, and he was ordered to poultice and foment it for a day or two. He then returned to have it dressed every day. On the 7th of July, (12 or 14 days after the accident), he complained of sore throat, with slight difficulty of swallowing. There was slight redness of the fauces, and his countenance was observed to be anxious and contracted. Next day he did not appear at the usual time to be dressed, and his residence not being known, he was not visited.

On the forenoon of the 9th, I received intimation from Mr Crichton, surgeon at Stockbridge, that the boy, who resided in his neighbourhood, laboured under all the symptoms of tetanus. On inquiry, it was found that some symptoms had appeared even so early as the 5th of the month. He had heated himself very much on the 7th, after having had the wound dressed, in running after some show or other. On his return home, stiffness of the jaw and threatenings of general convulsions made their appearance.

On visiting him, the spasms were found to be exceedingly severe, the jaw clenched, and the head and trunk drawn back very much. When taken out of bed, he was quite stiff, and seemed to have but little use of his limbs. The wound looked perfectly healthy.

The amputation of the hand was proposed, agreed to, and immediately performed. The bleeding from the previous wound was but slight. We wished to have abstracted a good deal of blood; but though the stump was held over a basin for that purpose, before any vessels were tied, for nearly 10 minutes, a sufficient quantity could not be obtained, not more than ℥viii. Could this arise from the coats of the vessels partaking in some measure of the general rigidity and contraction? One vessel only was tied, and the flaps brought together, and retained by compress and bandage.

The branch of the median nerve going to supply the thumb was found torn two-thirds across, and its extremity inflamed and thickened for nearly an inch.

A dose of calomel and opium, 2 gr. of each, was immediately exhibited, and 2 gr. of the former and one of the latter ordered to be continued every hour.

The bowels had been opened previously by medicine, and they were kept so by injections containing castor oil and spirit of turpentine. A blister was also applied to the back of the head and spine. The opisthotonos had next day totally gone off; the jaw could be more easily opened; the patient had slept well, without much starting; the motion of the limbs was quite free. In fact, in every respect he was so well, that very sanguine hopes of his recovery were entertained.

He became obstinate, and could scarcely be prevailed on to take any of his medicine. Spasms returned, more especially in the lower extremities, and he complained much of pain in the right groin. The stump was dressed on the 3d day, and looked well. In a few days after the operation, startings of the whole body returned. His countenance again presented the same look of distress and anxiety, and altogether there was evidently considerable loss of strength.

Wine was given him in such quantities as he could be got to take, but to no purpose. He died on the 17th of July, nearly three weeks after the accident, and eight days from the amputation.

Had the removal of the part been performed earlier, before the disease had been completely established, and taken such a hold of the constitution, in all probability complete success would have followed it. The relief afforded by the late performance of the operation, seems in some measure to warrant the supposition.

The body was carefully inspected, and appeared sound in every respect. The nervous system was natural, nor could any change be perceived, excepting in the median nerve at the bend of the arm, about 2½ inches of which was very vascular. No effusion had however taken place, nor was there any thickening of the nervous envelope.

VIII.

Cases of Hernia. By JOSEPH SWAN, Surgeon, Lincoln.

Strangulated Ventral Hernia.

MRS ——, æt. 37, had an irreducible hernia about the size of a small hen's egg, for several years. I operated on it on the 11th day after it became strangulated. I was informed that the original tumour was of the same size it had been for a great length of time, but that it had been inflamed several days, and in the last two had assumed a purple appearance, with a separation of the cuticle. Beneath this was another swelling, as large as a common sized tea-saucer, so that the hernia had altogether a very unusual appearance. I undertook the operation with no other view than that of giving vent either to matter or feces, as I was certain mortification of the intestine had taken place. After having made an incision like a T reversed, I divided the *fascia superficialis*, which formed a most distinct covering. I found a leech had bitten through this quite to the hernial sac. Before the sac was divided, an opening was discovered on its left side, which penetrated into the gut, and had been formed by the separation of a slough. As the opening into the intestine was close to the aperture through which the hernia protruded, the intestine was freely opened, without meddling with the stricture, as it appeared that any interference might allow the feces to escape into the abdomen. A considerable portion of intestine and omentum formed the larger tumour, and a portion of omentum, having the appearance of pancreas, the smaller one. Though the omentum had no unusual feel, it appeared mortified, and therefore was removed, when the blood in the veins was found to be coagulated. The patient showed marks of great debility before the operation, but it was supposed to afford the only chance of relieving her. She kept getting gradually worse, and died about six hours after the operation.

On opening the abdomen, the stomach and portion of the intestinal canal above the stricture were distended much beyond their usual size, though very little feculent matter was contained in them. The peritoneum covering the distended intestine was inflamed and coated with coagulable lymph. Feces * were contained in the abdomen, and had gravitated to the pelvis. The villous coat of the stomach and intestines was sound nearly as far as the hernia, when several ulcerations were observed on it, which I supposed to have been produced by a quantity of quicksilver, which was collected at this part. The liver was sound, but the gall-bladder contained one hundred and seventy-two calculi, which were white, and for the most part of the size of cherry-stones. The gall-bladder was completely filled by them, and one large one was fixed in the cystic duct. On examining the hernia, the intestine and omentum were found to have adhered entirely to the aperture at which they were protruded, except where the intestine had sloughed, and the opening produced by the separation of the slough was situated partly within the abdomen, and partly in the hernial sac. A portion of intestine near that contained in the hernia, was much diminished in size, and had been the cause of obstructions, one of which I was informed continued three days.

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* She complained of uneasiness about the rectum before the operation, and I was then afraid feces had escaped into the abdomen; for I have known the same sensations complained of, where matter had formed in the abdomen, and was found after death to have been principally contained in the pelvis.

Case II.—Mrs Barnard, æt. 58, has had a ventral hernia for the last seven years, for which she has worn a truss; but as far as I could learn, it had not been altogether reducible for several months. She had occasion to get out of bed on the night of the 20th of June 1822, at which time she was perfectly well; but before she got in again, she was suddenly seized with violent pain in the hernia, which was followed by vomiting, eructations, and pain in the abdomen. I saw her for the first time on the 22d, forty-one hours after the symptoms came on. I bled her, and attempted to reduce the tumour, which was large; and during this attempt it appeared to diminish; and as I had been told it had not been altogether reducible for some time, I hoped that purging medicines would relieve her. On the morning of the 23d, she had not passed any stool; and as she did not appear worse, I desired her to continue the medicines, and use purging clysters. In the evening, all her symptoms were much aggravated; therefore, at 12 o'clock, with the assistance of my brother, I performed the operation.

I made an incision through the skin in the manner advised by Sir Astley Cooper. I then divided the covering formed by the *fascia superficialis*, which was very distinct, and in some parts quite tendinous. I believed that there were adhesions between the sac and its contents, and therefore thought it most prudent to divide the stricture without opening the sac. After dividing the superficial fascia, I felt for the margin of the opening, which I freely dilated. The greatest part of the tumour was then easily reduced. The edges of the skin were brought together with several interrupted sutures and sticking plaster, and a compress of linen was confined on the part by a bandage. She was immediately relieved, and the vomiting and hiccup ceased.

24th.—At 2 *a. m.* she began to complain of great heat in the abdomen, and was restless. At four the pain in the abdomen was severe, and much increased on pressure. Twelve ounces of blood were taken from the arm, which relieved her. She was desired to take sulphate of magnesia dissolved in peppermint-water. At eight o'clock she had a clyster, which caused her to pass some feces and flatus. She had a great dislike to the sulphate of magnesia, and was therefore ordered to take half an ounce of castor oil every hour. By eight *p. m.* she had five stools. She was much better, but as the blood taken away in the morning had a buffy coat, and she had rather more pain than she had a short time before, eight ounces more blood were taken from the arm, which afforded considerable relief.

25th.—Ten o'clock *a. m.* She has purged several times, and is much better. The abdomen is soft, and much diminished in size, and is not painful when pressed. At seven *p. m.* for the last two hours she has been hot and restless, with some pain in the abdomen, especially on the right side. Twelve ounces of blood were taken from the arm, and twelve leeches applied to the abdomen.

26th.—She is better. After this she recovered gradually. The wound healed very well, and before the end of a month a truss was applied. The whole contents of the hernia entered the abdomen, so that a few days after the operation there was not the least tumour remaining.

Congenital Hernia.

James Else, æt. 45, had an inguinal hernia on the right side, which became strangulated on the 24th of June 1822. Purging medicines had been given him; he had been bled twice from the arm; turpentine enemas had been injected, and likewise two pints of an infusion from two drachms of tobacco; and cold water had been poured on the tumour. He was brought to the county hospital on the 27th. At this time the tumour did not feel very tense, but the skin was very much inflamed. The abdomen was not much enlarged, and was not very tender when pressed, but it was very tense. His pulse was quick and feeble, and he had continual vomiting.

I performed the operation at four *p. m.* without attempting any other means, as seventy-two hours had already elapsed. Very little fluid was contained in the sac, but there were about ten inches of ilium which was highly inflamed and very much distended. After dividing freely a stricture at both the external and internal openings, there was great difficulty in returning the intestine even after it had been nearly emptied, the portion of mesentery was so large and so much thickened. The testicle was situated just below the abdominal ring, and behind the hernia. The edges of the wound were brought together by means of interrupted sutures and sticking-plaster; a solution of sulphate of magnesia in peppermint-water is to be given.

9 o'clock *p. m.*—He has not vomited since the operation, and has been quite easy. He has had one small liquid stool; he feels pain when the abdomen is pressed; eight ounces of blood were taken from the arm.

28th, 9 o'clock *a. m.*—He has had several loose stools; the abdomen feels hard, and is tender when pressed. Twelve ounces of blood were taken from the arm, and twelve leeches applied to the abdomen.

1, *p. m.*—The blood was much inflamed; his extremities were cold; his pulse was extremely small and quick; he was delirious and appeared sinking.

29th, 5 *a. m.*—He is still delirious, but there is more warmth over the body, and he has had a very copious stool. He continued rather better for a few hours; he then became worse again, and died at four *p. m.* just forty-eight hours after the operation.

Examination.—On opening the abdomen there were very slight marks of peritoneal inflammation, and there was a very small quantity of purulent matter. The portion of intestine and mesentery had a better appearance than at the time of the operation, but they retained the most distinct mark of the stricture, and the bowel was so changed at this part as not to be capable of admitting the point of the little finger. The rectum was highly inflamed and ulcerated. As the vomiting did not return after the operation, and the patient purged copiously, notwithstanding the unfavourable appearances at the time of operating, I had hoped he would recover; and I think I should not have been disappointed if the intestine had not been permanently contracted. I am much afraid mischief is frequently done by the use of very hot clysters, and the appearances of the rectum led me to fear that the applications in this instance might have been too irritating.

Case II.—Mr Harvey, æt. 70, had an inguinal hernia on the right side for several years. It became strangulated on the 12th of November 1820 at 4 *p. m.* and I saw him at 9, when I in vain attempted to reduce it. He had a quick pulse and was in great pain, and the tumour was very tender. I bled him and applied cold to the tumour. About a quarter before eleven he had a clyster of half a drachm of tobacco infused in half a pint of water. He immediately fell asleep; in fifteen minutes he awoke, but was almost insensible; he had difficult respiration, and was covered with a profuse cold perspiration; every part of the body was in a very relaxed state; the pulse was weak, but distinct; though he continued in this state two hours, the hernia could not be reduced;—when the effects of the tobacco had gone off, sixty drops of laudanum were given him.

13th, 10 *a. m.*—The size of the swelling was much increased, and it had assumed a light purple appearance; he had vomited a few times, but there was neither hiccup nor tenderness of the abdomen. I immediately performed the operation. When the *fascia superficialis* was divided, there was another covering, but it was so indistinct that I cut through it and the sac formed by the peritoneum at the same time; about four ounces of red fluid gushed out; about eighteen inches of intéstine in two folds had escaped, and, with one of them, very much of the mesentery, and both these parts were of a dark red colour; the stricture was formed by the external ring, which it was necessary to divide largely before the parts could be returned; the testicle was situated just below the ring, and had never descended into the scrotum; the wound was closed by interrupted sutures and sticking-plaster.

After he was put to bed he became quite easy, and had a very profuse perspiration. Sulphate of magnesia dissolved in peppermint-water was given him. In the evening he began to purge, when the cramp came on to such a degree that it became necessary to give him an opiate, which stopped the action of the purging medicine.

14th.—He had no complaint; he took castor oil, which purged him in the evening.

15th.—His pulse was 100, and had a jirk. He had great tenderness of the abdomen. Twelve ounces of blood were taken from the arm, and castor oil was given him. In the evening he had a clyster, which purged him copiously, and afforded great relief. The pulse had not the jirk; the blood had a buffy coat.

16th.—He was better, and the tenderness of the abdomen was much diminished.

17th.—I was called up to him, as he supposed the bowel had come out again through the wound; the dressings were disturbed, and it appeared that the testicle had been mistaken for a portion of intestine.

18th.—He had hiccup and some wandering, but in other respects he was better. He was so weak that it became necessary to give him wine; in a month he was able to wear a truss, and was perfectly recovered.

IX.

Case of Melæna, with Dissections. By Richard Martland, M. D. & M. R. C. S. L. &c.

H. Johnson, æt. 49, a tailor, has led a very irregular life for several years, and been much addicted to dram-drinking. He has generally enjoyed good health till within the last twelve months. He is reported to have vomited a small quantity of blood about the middle of last May, after drinking ale.

On the 16th of July 1823, I was first called to see him, on account of the unnatural appearance of his alvine evacuations, which were of the colour and consistence of liquid pitch, remarkably fetid, but in small quantity. He had no pain in the abdomen, but felt an oppressive weight about the præcordia; his strength very much impaired; his countenance pallid, and very anxious; pulse about 110, and weak; skin hot and dry; urine high coloured, and in small quantity; vertigo, without any positive pain in the head.

He was ordered a saline cathartic, and afterwards powders composed of nitre and the compound powder of ipecacuanha, which were continued until the 21st, when most of the symptoms were relieved, but there was no perceptible change in the colour of the stools. He then began taking a mixture composed of nitre, sulphate of magnesia, and sulphuric acid. On the 26th, his stools, which were before in sufficient quantity, began to change in colour, and on the 28th were nearly natural. At this time a dropsical effusion, which had only been observed for two days, had taken place to such an extent in the abdomen and lower extremities, that he was ordered to take, in form of pills, a combination of the mercurial pill, elaterium, foxglove and opium, using at the same time a solution of nitre in infusion of juniper tops.

By a continuance in the use of these medicines till the 10th of August, his dropsy was quite removed, and his only remaining complaint was debility, of which he very soon recovered.

From what I had now seen of the case, I had determined, should a similar one occur, on using the following medicines.

℞ *Magnes. sulph.* ʒ*vj. Acid. sulph. dil.* ʒ*iss. Aquæ font. lbss. M. ft. mist. cuj. capt. coch. ij. amp.* 3 *vel* 4 *hor.*

℞ *Hydr. submur. gr. xij. Pulv. ipec. gr. vj. Opii gr. iij. Ext, tarax. q. s. ft. pil. xij. quar. sum. i. n. m.*

On the 2d of October I was again called. His complaint had now returned in an aggravated degree; his stools were of the pitchy colour and consistence, and very offensive, but in small quantity; his face was fully paler than at the beginning of his former attack; his eyes were dull; had some pain in his head, and much vertigo; pulse 130, very weak; tongue foul and dry; skin hot and parched.

Was ordered the pills and mixture as above mentioned.

Early on the morning of the 3d, he vomited about half a pint of black grumous blood, and became quite comatose. When I saw him, his pulse was fluttering, and scarcely perceptible; forehead covered with a cold clammy sweat; his extremities cold; eyes sunk, and countenance extremely pale; breath very offensive. In this state he continued all day, without being able to swallow; but occasionally black grumous blood was discharged from the mouth, and pitchy matter from the bowels. At 8 *p. m.* he expired.

Alum-whey glysters were used during the day without effect.

Post-mortem Examination.

Owing to peculiar circumstances, the body could not be examined till 48 hours after death. Mr Surgeon Bailey and our pupils were present at the examination.

We found the smell unusually bad, the corpse extremely blanched, and the abdomen much inflated; black grumous blood had issued from his nose and mouth since death; and by pressure on the belly, it was forcibly discharged, along with a very fetid gas.

The cellular substance covering the abdomen was fully an inch and a half in thickness; and the omentum, much distended with fat, covered the whole intestines, and adhered to the *fundus vesicæ.* The whole of the alimentary canal was distended with very fetid gas. The stomach contained about half a pint of black grumous blood at its left extremity; there was but little matter in the intestines, and it was similar to that before discharged. Nothing like proper feces could be found. The peritoneal coat was quite transparent, and showed the villous throughout the whole canal.

The villous coat of the stomach was inflamed in every part. It assumed a gangrenous hue at the cardiac and pyloric extremities, but was least affected in the vicinity of the pylorus. In the duodenum the inflammatory appearance was but trifling, but this increased gradually as we approached the ileum, where it seemed as in the worst state of gangrene; no effusion, however, could be detected between it and the other coats, either here or in any other part of the canal. From the termination of the ileum to the descending branch of the colon, were progressively exhibited different appearances, from the gangrenous inflammation to a blush. The left colon, and nearly all the sigmoid flexure, had a healthy look, but the villous coat of the rectum was nearly as much affected as the cardiac extremity of the stomach. The mucus lining this coat was very viscid, and of the same colour as the part of the membrane on which it lay.

A large quantity of fat was found in the mesentery, mesocolon, and about the great intestines.

The liver was of a pale brown colour, smaller than usual; and in place of a smooth polished surface, it exhibited a rough shrivelled appearance, with indentations and fissures one-eighth of an inch in depth. It contained neither blood nor bile, but was of a natural consistence. The gall-bladder was nearly empty; the bile was browner than usual. The spleen contained little blood, but was of the natural size and appearance. The kidneys were flabby; in other respects healthy. No effusion was found in the abdomen.

The lungs were sound, the heart quite devoid of blood and flabby; neither was there any blood found in any of the larger vessels. Three or four drachms of serum lay at the base of the brain; it was otherwise healthy, but paler than usual.

Blackburn, 17*th November*, 1823.

X.

Observations upon the Effects of Cubebs in Gonorrhœa, Fluor Albus, and Chronic Rheumatism. By W. Crane, M. D. Boston, Lincolnshire.

Gonorrhœa.

The attention of the medical practitioner to the superior efficacy of cubebs in the cure of gonorrhœa, or, as it ought to be more properly termed, blenorrhœa, was first excited by a paper of Mr Crawford's in the Edinburgh Medical Journal; and afterwards more fully by a pamphlet of Mr Jeffreys, entitled, " Practical Observations on the Use of Cubebs, or Java Pepper, in the Cure of Gonorrhœa. " From the success which attended its administration, as detailed in Mr Jeffreys' work, I have been induced to prescribe it freely in my own practice;

and the result has been, in general, beneficial; but not so universally effectual in removing the disease as to allow it to be entitled a never-failing remedy or specific.

The effects of cubebs afford considerable variety in different individuals. In most instances, after a few doses have been taken, there is a more frequent desire to make water, and the urine becomes strongly impregnated with the peculiar odour of the medicine; the discharge decreases, and the heat of urine is much relieved. When the gonorrhœa is slight, some patients have complained of the cubebs exciting a peculiar itching in the *glans penis*, accompanied with a great desire "*venerem appetere.*" The cubebs also occasion heat in the rectum and in the perinæum. Generally, the bowels are kept gently open; but in some patients an opposite state is produced; and in that case it is proper to give a few grains of the mercurial pill every other night at bed-time, and a small dose of some aperient salt in the following morning. Mr Morley, in the London Medical Journal for January 1821, states, that small doses of submuriate of mercury, taken every night at bed-time, aid the beneficial effects of the cubebs.

This medicine, in most instances, improves the appetite, and somewhat exhilarates the spirits. In two cases that I attended, it produced very considerably mental irritation and anxiety; the irritation produced in one of these was so great, that the patient was compelled to relinquish taking the cubebs. Both these patients were of a sanguine temperament. Sometimes the cubebs, when given in very large doses, occasion great nausea, and even vomiting. But I never observed any other unpleasant consequences, except the irritation of mind just mentioned. Small doses of cubebs in powder are of little or no service. They must be given in doses of from two to three drachms each, three or four times a day, as the stomach may be able to bear. I generally give the powder in cold water; some patients take it better in milk and water. Some advised coffee; but my patients declared that both the cubebs and the coffee were spoiled, and preferred taking the powder in cold water. I have also prescribed cubebs mixed with honey as an electuary; but this form is inconvenient, as it is apt to create sickness. Few patients, indeed, are able to take of this mixture a sufficient quantity for a dose. The best mode of prescribing the cubebs when in powder, seems therefore to be this, viz. to take the powder in cold water, or in milk and water, or any other mild liquid, as may be the most agreeable to the patient's stomach. After taking the third dose, the patient's water begins to smell very strong of the cubebs, and the discharge from the urethra becomes less consistent; at the end of the second or third day, the disease, if very recent, almost entirely disappears. When the gonorrhœa is removed, the cubebs should be still continued for ten days or a fortnight, the patient gradually diminishing, in the course of each day, the frequency of his taking the doses. If this continuance of the medicine, and gradual diminishing of the frequency of taking it, be not carefully attended to, the discharge and all the other symptoms are apt to return. It not unfrequently occurs, that, when the cubebs have been taken in large doses for a considerable length of time, there is a thin and nearly colourless discharge from the urethra, which is small in quantity. This discharge may be distinguished from the true gonorrhœal discharge, by its being of a bluish white colour, and by its not imparting any peculiar colour to the linen of the patient. This will not be removed by continuing the medicine longer; but, if it be omitted, and a weak solution of the sulphate of zinc be injected two or three times in a day, it will shortly disappear. It is not improbable that this discharge is produced by the irritation which the cubebs excite in the course of the urethra. And perhaps it will not be wrong to suppose, that the efficacy of the cubebs in gonorrhœa may arise from a counter-irritation (if such an expression may be admitted), incompatible with the inflammatory excitement arising from that disease. That a peculiar irritation is excited by the cubebs, may easily be proved, by taking, when in health, for a week or ten days, the full doses as recommended above, for the cure of gonorrhœa.

During the time of taking cubebs, it does not appear necessary, when they are given in the earliest stage of the disease, to restrict the patient to any peculiar diet. But if it has made considerable progress, the patient ought carefully to guard against those causes which may produce irritation. For this reason, he ought to avoid taking strong exercise, abstain from drinking wine or spirits, and to be very sparing in the use of animal food. If, after having taken the cubebs for six or eight days, no effect be observed in arresting the progress of the disease, it is useless to persevere in taking them any longer. In one instance, I found advantage in giving to the patient a full dose of cubebs in the morning; and the balsam of copaiba twice a day, in form of an emulsion, according to the prescription by Dr Armstrong, in his " Observations on the Efficacy of the Balsam of Copaiba in Inflammations of Mucous Membranes. " When the cubebs were omitted, the patient stated that he had felt, during that day, more of the *ardor urinæ.* Cubebs in most instances relieve the complaint, and very few examples occur in which the medicine altogether fails. According to the statement of Mr Broughton, in the Medico-Chirurgical Transactions, vol. XII., in fifty cases in which this remedy had been employed, he found three patients on whom it produced no sensible effect. A few months since, I met with a case, where the cubebs entirely failed of success in checking the disease. This patient took, four or five times a day, two drachms and a half of the powder, which created a constant nausea, and very considerable mental irritation. He then took the balsam of copaiba; but this caused so much sickness, that he declined repeating it after the third dose.

The cubebs sometimes appear slow in removing the disease; which slowness arises, not from any fault in the remedy employed, but from the irregularities of the patients, many of whom are too apt to indulge, whilst taking it, in that evil company which was the original cause of their complaint.

The tincture of cubebs has been spoken of by some writers as nearly equal to the powder, in curing gonorrhœa. From the few trials I have made, I am inclined to think the powder preferable to it. A combination of cubebs, with balsam of copaiba, has lately been recommended, which forms, in my opinion, an exceedingly nauseous medicine. I prefer giving them separately, as mentioned above.

Leucorrhœa, or Fluor Albus.

When leucorrhœa occurs from an increased action of the lacunæ of the vagina, or inner membrane of the uterus, cubebs will probably be a useful remedy in removing the disease, or when it takes place in females of a delicate habit, after an excessive flow of the catamenia, without any organic disease of the uterus. But in the latter cases the employment would, in all probability, be, on account of their stimulating property, rather hurtful than beneficial.

The good effects of cubebs appear to be increased, when a few grains of magnesia are added to each dose. I have generally given a drachm or a drachm and a half of the cubebs, with eight or ten grains of the magnesia, three times a day. I have indeed prescribed larger doses, but I did not find that any advantage was derived from the increase of the quantity. A drachm being in fact nearly as much as the stomach of a delicate female can bear. Dr James Orr, in his "Cases of Leucorrhœa," cured by this remedy, gave it in the same dose; and Dr Trail, in his paper "on the Utility of Piper Cubebs in Leucorrhœa," published in the Edinburgh Medical Journal, vol. XVII., states, that he found many patients who could not take more than a drachm in a dose.

Chronic Rheumatism.

This disease, in most instances, may be considered as incurable, and the unfortunate sufferer is doomed to lead a life of pain and uneasiness, which imbitters his hours by day, and deprives him of rest at night. Fearfully he views each passing cloud, and, sensible to every change of weather, he becomes a living barometer. Happy indeed would it be, and great would be the joy of thousands, if a plan of cure should be devised for those who are afflicted with chronic rheumatism. This consideration encourages me to hope, that I may be permitted to state the effects of a remedy, which has at least, in a few instances, proved beneficial.

Accident led me to the employment of cubebs in chronic rheumatism; and in several cases it has afforded great relief, although in others it has produced no effect whatever.

The cubebs were prescribed for a gentleman, in consequence of having contracted a gonorrhœa, who had for several years previously been afflicted with chronic rheumatism. He observed to me, that whilst taking this remedy, his rheumatic pains were also very considerably alleviated. After the disappearance of the gonorrhœa, I advised him to continue taking the cubebs in drachm doses twice a day for several weeks. In the course of two months, he found that he was nearly free from pain, and that he could move his legs and arms with greater ease. During the following winter, he was much less troubled with his rheumatic pains than he had been during the last four or five years; and whenever he feels any return of his complaint, he takes the cubebs for a few weeks.

A young woman, 20 years of age, who had been under my care about two months, on account of the pain she suffered from chronic rheumatism, deriving no relief from the remedies employed, I determined to prescribe for her the cubebs. She took them in half drachm doses three times a day. On the third day, she said her pains were much less severe; and in six weeks, she was completely free from her complaint, and able to return to her service.

In some cases of chronic rheumatism, I found the cubebs to be apparently useless. The cause of this failure of success might, perhaps, in some of the cases, be accounted for, not from any fault in the medicine, but from a want of perseverance in the regular use of it; as we often find, that when a remedy is to be continued for many weeks, it is taken irregularly, and even omitted for a day or two without the medical attendant being informed of such omission; and then upon the remedy falls that blame which is due only to the patient and his friends. This is not unfrequently the case amongst the lower classes of patients, who have little faith in any medicine which does not act both effectually and speedily; and who are very apt to tell their medical attendant they are regularly taking the remedy which he has prescribed for them, when the truth is they are not doing so.

XI.

Medical and Meteorological Notices, with References to the State of Health and Disease in Newcastle-upon-Tyne, from December 1822 *to December* 1823. By Henry Edmondston, A. M. Surgeon.

The last twelve months have not been productive of much that is interesting in regard to disorders supposed to depend upon atmospherical causes. I shall therefore spare your readers the trouble of much minute monthly details, or tables of the weather and other phenomena. The whole year, excepting the greater part of last month (November) has been in a remarkable degree austere and unfriendly—the winter long and inclement, though not intensely cold—spring ungenial and very late—the summer passed away with not more than two or three hot days—and autumn was cold, wet and gloomy. Yet vegetation in general, especially the hardier kinds of it, although retarded in its progress, did not at any time appear to suffer materially. All sorts of fruit, however, ripened very imperfectly.

The year 1822 closed with violent winds, and harsh, though not very cold weather, most flowers and shrubs, and particularly evergreens, appearing more healthy and vigorous than usual. The heavy storm which had visited us pretty smartly about the middle of January this year, and which in the beginning of February prevailed so universally, was felt with peculiar severity in this part of the kingdom, to the distance of from thirty to sixty miles north and south of Newcastle: Up to the 10th of the month, there fell an uncommon quantity of snow, with violent wind from the north-east; and as the whole eastern part of Northumberland is low, flat and unsheltered, the snow became still more formidable from its drifting. For seventy hours together, snow or hail continued to fall, without any interval of fair weather of more than ten minutes. Its average depth on the ground might be about twenty-four inches, but in many places the drift snow lay twelve and fourteen feet thick. To the northward in particular, much serious inconvenience was occasioned by obstructed roads, not only to single households, but to whole villages, from the want of fuel, medical assistance, &c.; and many unfortunate persons perished in the snow. In the higher parts of the county, towards Scotland, the sheep flocks suffered considerably. A good many wild swans appeared in different places, and some were shot; and more wild ducks were seen than any one remembered. Several beautiful specimens of the snowy owl *(Strix Nyctea Linn.)* were shot; a circumstance perhaps unprecedented, and only to be expected in seasons the most rigorous. The snow continued to lie for a long time; and though the thaw was hastened by rain and sleet, it was so gradual that March was far advanced before the ground was cleared, and even after that there was a good deal both of rain and snow. The weather during the latter month was, upon the whole, favourable to the country, although not to such a degree as to counteract the effects of the long continued and heavy snows of the preceding months. April was cold and windy; vegetation very backward, nearly a month later than last year. Until about the 5th of May, the appearance of the country was very unpromising. After that it improved considerably under the influence of mild rains, but still with variable and by no means warm weather afterwards. Swallows were long in arriving, and not very numerous. Swifts seemed to be rather more plentiful. The cuckoo was very little heard. If newspaper statement may be trusted, this harbinger of summer must have lingered long with us this year, for it is said to have been heard in our neighbourhood not more than two months ago. June was unusually cold and boisterous, with much dry easterly wind and very little sun, presenting a striking contrast to the corresponding month of the foregone year. The 10th of July might be called the first really summer day; but the month altogether was cool and cloudy, and after the 15th (St Swithin's), not one completely fair day occurred, nor indeed till far on in August. Insects did not abound. Though the latter half of August, and through to the middle of September, were good, and rather friendly to vegetation, yet the temperature was greatly below the average standard, with unsteady weather towards the end of the latter month, and longer continued rains than usual; notwithstanding which, the springs were observed to be very low, and the country dried very rapidly.* Owing to some piercing frosty nights and strong winds, the trees began to shed their honours very early in October. Woodcocks came over to our coasts in considerable numbers, and it was said fieldfares. November has been favourable to a degree scarcely before remembered. With the exception of a few blasts of wind, one of them indeed almost amounting to a slight hurricane, the weather, from the 9th to the 29th of the month, was mild, calm, and without one drop of rain; the temperature singularly steady night and day, and the barometrical range higher than it has been during any month of the year. The verdure of the fields, which had early become faded, was soon restored, and every kind of husbandry labour has been carried on uninterruptedly. This state of weather has, within the last ten days, given place to some hard gales, with one very heavy fall of rain and sleet from the north-east. The temperature of the air, however, though more suited to the advanced period of the year, is not much reduced; the days are fair and sunny, the nights clear, and the season, upon the whole, open and agreeable.

The fever which prevailed extensively during the autumn of 1822, declined very fast after November. The admissions into the Fever Hospital, which that month were eleven, sunk in December to one; and though they mounted up to seven in January of this present year, they have averaged little more than one for each of the other months. The whole number of cases of well-defined fever admitted this year have been fourteen, while last year forty-two admissions took place in the space of three months only. No patients were received during last month, and at present the town may be said to be as nearly as possible exempt from fever. Nor has the healthiness of the place in other respects been less observable, hardly any interruption to it of importance having taken place, unless from the usual visitants, or, I should rather say, residents, viz. scarlatina, hooping-cough, and measles. These are continually showing themselves more or less, but without having exhibited in their character or progress any thing that demands particular notice on the present occasion. The pains in the joints resembling rheumatism, which I formerly pointed out as sequelæ to scarlatina, continue, so far as my observation goes, to occur in every case. In one instance, there was superadded abdominal enlargement, with great derangement of the alimentary and digestive functions, which, in its symptoms, has proved very anomalous, and in its progress and treatment, tedious and difficult in the extreme, and even yet, convalescence is far from being firmly established.

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* This fact is more particularly the result of the observations of my friend James Losh, Esq. of Jesmond Grove, who has remarked it on many former occasions. To this gentleman I have great pleasure in acknowledging my obligations for the assistance I have derived from his excellent journal (the only one, I believe, kept with any degree of regularity in all this part of the country) in correcting my otherwise very imperfect notes on the subject of climate.

About the commencement of the year, there presented a more than ordinary number of cases of jaundice, and somewhat simultaneously, in situations much asunder, and where no community of cause or circumstances was traceable. They were, for the most part, mild, and without pain, although, in one of them, the disease was ushered in with agonizing pain in the stomach, and vomiting, which lasted a couple of days. They yielded very easily to the usual treatment.

About the same time, a patient was attacked with pain over the loins, from over-stretching the muscles in raising a very heavy weight, without the necessary purchase. The pain, though in general more diffused, was in kind identical with that occasioned by pure lumbago, and, on certain movements, was quite as circumscribed. It soon got well, with very slight treatment.

For a short while, in the early part of spring, catarrhal complaints, occasionally assuming a severe form, particularly in old people, were observed to prevail somewhat like an influenza.

It has struck me, that, in the course of the last year, I have observed, in the streets of this town, *en passant*, more hemiplegic persons than I have been accustomed to meet with, especially amongst the lower orders. There appear also to have occurred in the middling classes, many cases of indisposition (active, well-marked disease it could scarcely be called) connected with the cerebral functions. The symptoms are very various, fluctuating, and ill defined: a few of the more prominent, perhaps, are giddiness, pain, and weariness of the muscles of the back of the neck, and a feeling as if the vessels about the base of the cranium were tender and corded, sense of fulness about the throat, ears and eyes, tinnitus, slight confusion of thought and perception, irregularity and frequent lowness of spirits, irritability of temper, lassitude, much discomfort and nervous ailment, &c. In the treatment of these affections, very perceptible benefit has, I think, been derived from the use of the *Ungt. antimon. tartarizat.*, according to the principle of Dr Jenner. A professional friend also informs me of a case, in which the good effects of this application were very decided and unequivocal, no other means whatever having been employed to which any part of the satisfactory result could be attributed. The patient, a lady somewhat advanced in life, experienced, in February 1822, a severe attack of regular epileptic disease, which recurred with undiminished force in May and September following, and again in January and March of the present year. The treatment, which had been that of moderate depletion, did not seem to exert much influence on the disease, either one way or other. Shortly after the March paroxysm, the use of the *Ungt. antimonii tart.* was begun, and has been steadily persevered in ever since; and it has so happened that, except an obscure threatening lately, which seemed to originate in indigestion, and which passed off in a few moments, the patient has continued in the enjoyment of perfect health. There has been the ordinary habitual attention, of course, to the state of the bowels; but beyond that, not a drug of any sort has been administered during the nine months interval, which has now been completed. *

In hooping-cough, and other pectoral complaints, the *Unguentum* has been found very serviceable; its effects, though not so immediate, appearing to be more permanent than those derived from blisters or issues, and its action more easily and equably kept up.

Cholera has been very little seen, even during its own proper season.

In the practice of our Infirmary, nothing very remarkable has been observed. Sarsaparilla continues to hold a high place, not only in that class of complaints to which it is believed to be more particularly adapted, but in a variety of others. For example, in cachectic states of the stomach, relaxation and superficial ulceration of the fauces and mouth, and more especially in those anomalous and obscure conditions of the constitution brought on by the too free and continued use or unsuitableness of mercury. † Hydrocyanic acid has had ample trials, and has been found to fail altogether in answering any of the confident expectations held out of its virtues. Much the same may be said of the colchicum. The arsenical solution ‡ has succeeded in almost every case of chorea in which it has been tried, a brisk purgation having been premised. My friend, Mr Moore, who is one of the surgeons to this excellent institution, gives me leave to state the following particulars of a case, which must be considered very rare. "A pitman, aged 19, going home intoxicated two years ago, (such was his own account), fell asleep in a field, and while insensible, his companions played him the mischievous trick of introducing through the urethra into the bladder, a small-sized horse bean; nor was he aware next day of what had been done, till they told him. He shortly after began to complain in making water, and in June last he was admitted into the hospital, with symptoms of stone, sounded, and a stone felt. On the 24th of that month, he was cut, and a loosely cohering rough-grained sandy stone extracted, which crumbled in pieces when grasped by the forceps. Nothing uncommon happened during the operation, and all the fragments were supposed to be removed, though not to the entire satisfaction of the operator. The after symptoms were very moderate; the patient occasionally complaining of the pressure of something hard in the wound. On the morning of the seventh day, a gritty substance was felt at the neck of the bladder, on introducing the finger, and some fragments of stone brought away by the forceps, together with one-half of a small horse-bean, and part of another half with a portion of the outer shell or covering. Next day the remaining part of the bean passed by the urethra. In five weeks the man went out well."

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* I ought to mention, that from various circumstances which have occurred in my practice, my attention may have been lately more awakened to these affections, than it used to be. It is also certain, that in one or two instances they were traceable to faultiness in the hepatic and alvine systems. Still they would seem to be not undeserving of consideration on their own account. With reference to this point, may it not be said that the old English custom of phlebotomizing once or twice a year has fallen too much into disuse of late? And amongst the many causes assigned for the supposed greater frequency now than formerly, of apoplexy, paralysis, and the rest of the neuroses, may not this be one?

† A similarly favourable report with regard to this medicine, I am enabled to give from the Journals of the Lock Hospital of this place.

‡ According to the plan of treatment suggested by Mr Salter, Surgeon, of Poole. Vid. Med. Chirurg. Transactions, Vol. X. Part I.

At this hospital the operation of lithotomy has been performed with success several times lately, with the knife introduced by that very expert and intelligent surgeon, the late Mr Benjamin Gibson of Manchester. In the last case, which occurred a month ago, a straight staff, differing very little from a female one, was introduced upon the common curved one, after the urethra had been perforated in the usual way, and the operation finished with the same knife. This mode of operating was found to be safe, easy, and expeditious.

Three cases of small-pox occurred in the dispensary practice. The disease did not spread. Vaccination maintains itself in despite of all the attempts so vehemently and perseveringly made by a certain northern antivaccinist, to shake public confidence in its efficacy.

Newcastle-upon-Tyne, 9th December 1823.

XII.

Description of an Instrument for passing Ligatures round deep seated Arteries, used in Physiological Experiments. By J. L. Prevost, M. D., Geneva.

In the 64th Number of the Edinburgh Medico-Surgical Journal, I saw Mr Liston's Paper on Ligatures. Having been engaged for two years past in physiological pursuits on blood and its action, I have had great experience in applying ligatures on blood-vessels very deeply seated. Much of the difficulty met with in such operations, is done away by means of an instrument executed for me a year ago, by Mr Selligue, a very ingenious mechanician settled in our town. As the method I follow may prove serviceable in surgical cases, I would be very glad to see it known through the medium of your Journal.

The vessel having been laid bare and sufficiently separated, the needle fig. 2, armed with the ligature *h*, is fixed between the blades *b* of the forceps fig. 1, and passed round the vessel as in fig. 4. The button *d* of the needle is then passed between the ring *g* of the shield, and the point of the forefinger of the left hand introduced into the wound. The blades of the forceps are now opened by drawing back the cannula *a*, and the needle is withdrawn by the finger. A knot is next formed on the ligature, and one end being passed through the little tube *i* at the end of the shield, the finger is again carried to the bottom of the wound, until it come in contact with the vessel, and the knot is then tightened. A second knot is lastly formed in the same way.

By proceeding in this manner, we may apply ligatures on blood-vessels very deeply seated, in making but trifling openings, compared to the depth we have to reach to. The operator's finger directing the needle, there is no fear of comprehending in the ligature, any thing but the vessel to be tied; and the operator's finger being so intimately connected with the instrument, by means of which, both the needle is drawn out, and the knot fastened, no parts run any risk of being bruised, as would be the case, were forceps made use of.

Fig. 1. represents the forceps I use; the two steel blades *bb* of the instrument slightly diverging, form an angle with each other; when the tube *aa* in which they are contained, is made to slip towards the handle; when, on the contrary, that tube is pushed forward, the extremities *bb* are brought into close contact.

Fig. 2. *c*, The needle to pass the ligature round the blood-vessel, terminated by a conical button *d*, which prevents the needle from slipping, when it is engaged between the operator's forefinger and the ring *g*, Fig. 5.

Fig. 3. *e*, The forefinger of the operator's left hand, *f* a thin plate of silver coinciding with the nail, and steadily retained in that situation by the rings *gg*, the diameter of which is very narrow. *i*, Is a short tube or hole through which is passed the thread *hh* for the ligature.

Fig. 4. *ww* is the incision made to reach the blood-vessel *vv*; the finger *e* on which is placed the thimble *f* (*v.* fig. 3.) rests on the vessel *vv*; the needle *c* has partly made its way round the vessel.

Fig. 5. The forceps *bb* opened. The needle *c* fixed between the ring *g*; the finger is drawn out, and the ligature *hh* is passed round the vessel *vv*.

Fig. 6. The first knot of the ligature has been formed, one of the threads *h* passed through the duct *i* (*v.* fig. 3.), the finger is in the way of fastening the knot *k*, by pressing on *k*. The two extremities of the ligature are in the right hand of the operator, and out of the wound.

Fig. 7. The knot steadily fastened.

Geneva, 7th November, 1820.

XIII.

A Description of a new Instrument for tying Arteries without the aid of an Assistant, invented by Professor LUDWIG JACOBSON *of Copenhagen, and communicated by him to the Royal Medical Society of that city in the year* 1817. By GEORGE WM. STEDMAN, M.D. late President of the Royal Medical Society of Edinburgh.

A MEDICAL man often finds himself in a situation where it is difficult to obtain proper assistance in performing operations. The tying of arteries, although by no means a matter of difficulty, is often embarrassing from the awkwardness or ignorance of the assistant. To obviate this inconvenience, many instruments have been invented, but as none of them seemed sufficiently to answer the end proposed, Professor Jacobson of

Copenhagen has invented one, which has met with the approbation of many experienced surgeons, and which he himself has used for many years.

The instrument as originally constructed, consists of an open thimble, which is made to fit the index finger of the left hand, and which reaches nearly to the second joint. Upon the point there is a prolongation of two lines in length, at the end of which are fixed two surfaces of somewhat a semilunar figure. The instrument is to be put on the index finger of the left hand; the ligature made into a running knot, is put into the concave depressions on the edge of the semilunar plates above mentioned, and the ends of the ligature are held by the thumb and middle finger of the same hand.

The artery is taken up with the forceps by the right hand, and the knot being laid over the mouth of the bleeding artery, the ends of the ligature are drawn upwards by the thumb and middle finger of the left hand, the lower end of the instrument serving as a fulcrum. The mouth of the artery being thus secured, the operator may use both his hands in making the second knot.

Professor Jacobson has lately improved this instrument, and has rendered it more portable by a simple contrivance.

In its improved form it consists of two legs, in which are fixed two moveable rings, which can be made to lie flat between these legs, or when used, to stand out at right angles, so as to form a sort of thimble.

The point of the instrument is also improved by a hinge, so that the half-moon plates formerly mentioned can be made to fold or unfold at pleasure.

The principal use of this instrument is to tie bleeding arteries so as to supersede the necessity for an assistant; but it may also serve to apply the ligature where the wound is of such a nature that the artery can only be got at with difficulty; for example, in the excision of tumours in the axilla, the extirpation of the eye, &c.

It may also be of great service in tightening the ligature round the artery in the operation for aneurism, where the depth of the wound often embarrasses the surgeon.

In cases of polypi in the fauces, within the anus or vagina, this instrument will also be found of great assistance in applying the ligature.

Description of the Plate.

Fig. 1. *a a*, *b b*, The thimble.

c, The columella or stalk.

d d, The semilunar plates forming the fulcrum.

Fig. 1

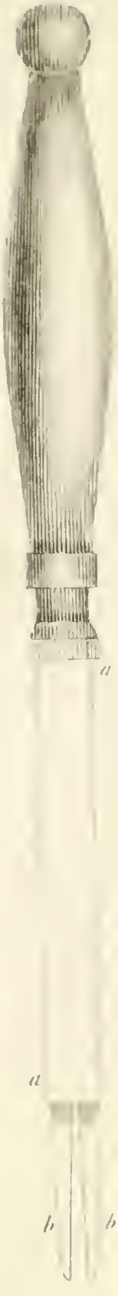


Fig. 3

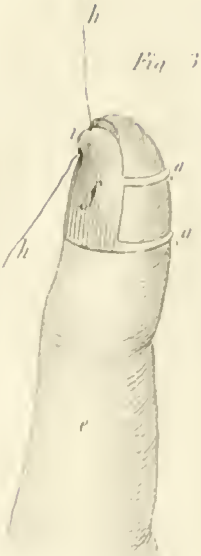


Fig. 4

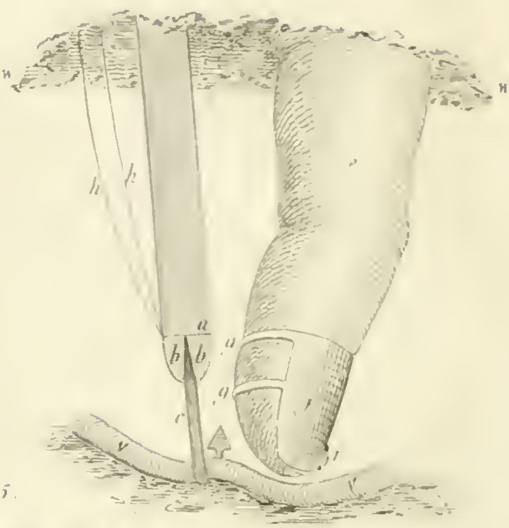


Fig. 5.

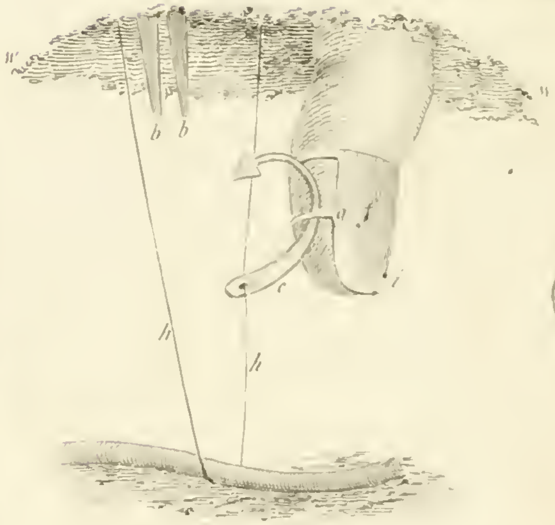


Fig. 2

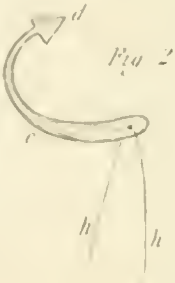


Fig. 6.

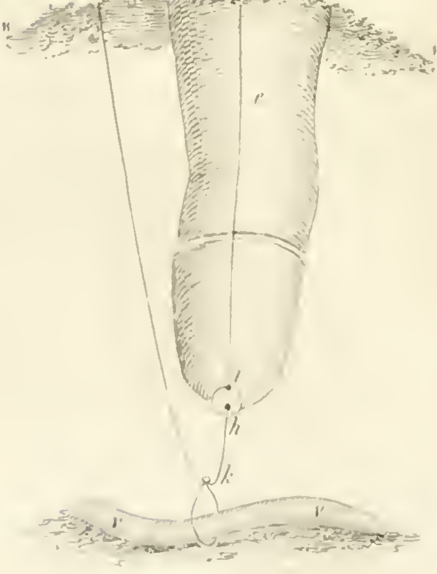
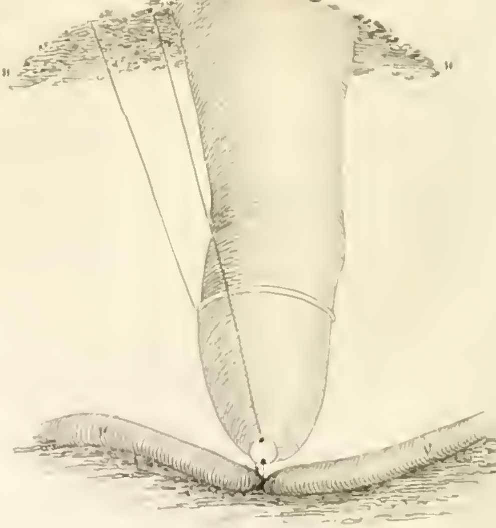


Fig. 7



PROFR JACOBSON'S INSTRUMENT

Edinr Medical Jour No 7[illegible] P. 315

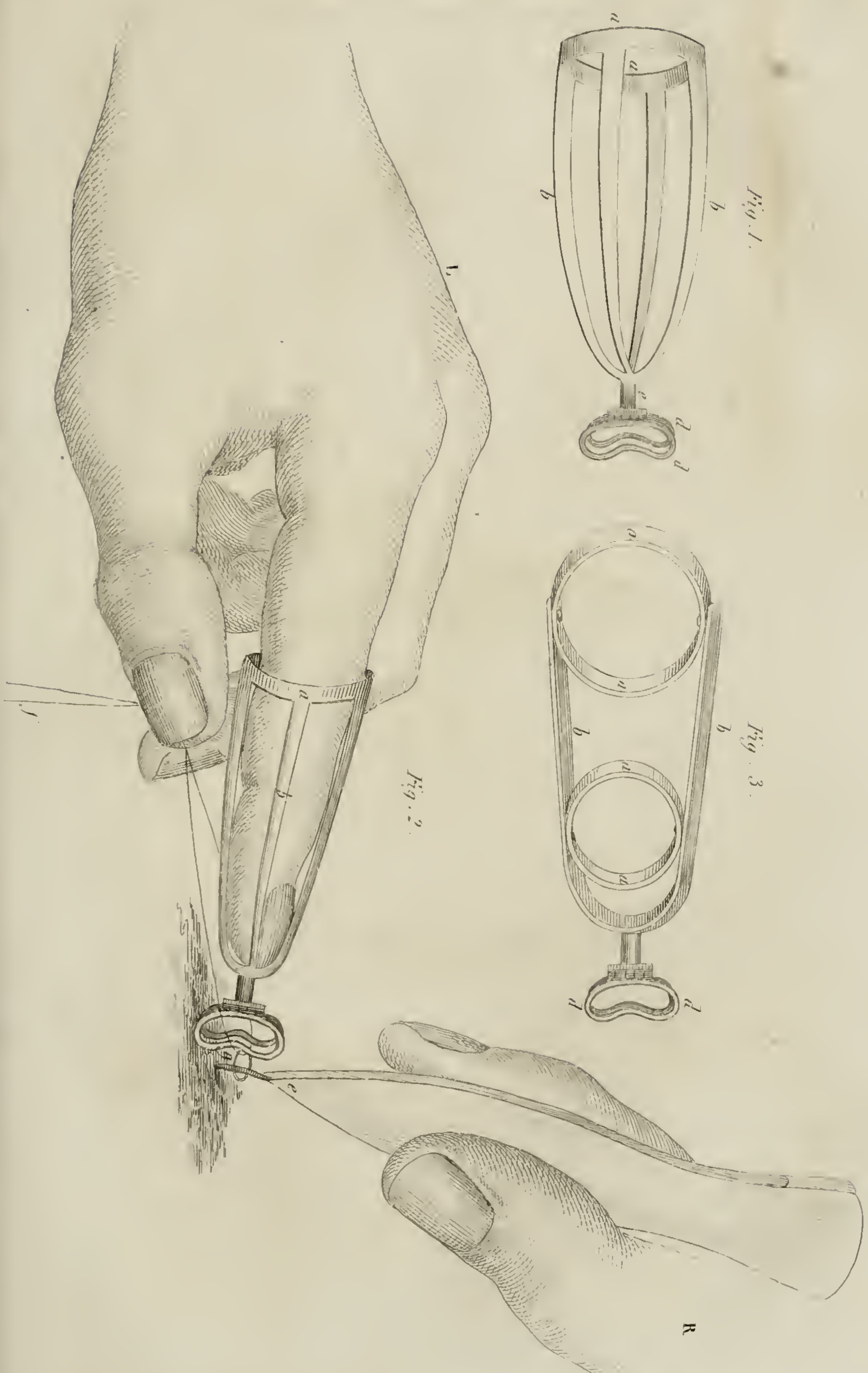


Fig. 1.

Fig. 2.

Fig. 3.

Fig 2. Represents the mode of using the instrument.

L, The left hand of the operator.

a b, The instrument on the index finger of the left hand.

f f, The ligature held by the thumb and middle finger of the same hand.

g, The running knot over the mouth of

e, The artery drawn out by the forceps.

R, The operator's right hand, with the forceps holding the artery.

Fig. 3. Represents the instrument in its improved form.

b b, The legs of the instrument.

a, *a*, *a*, *a*, The moveable rings lying flat between the legs.

d d, The semilunar plates folded together.

XIV.

Case of Obliterated Urethra, successfully treated with Lunar Caustic. By William Maclure, Surgeon, Glasgow.

Sect. I. *Introductory Narrative.*

D. —— N. ——, aged 40. Sixteen years ago this man contracted a gonorrhœa, and, at the same time, chancres on the lower surface of the glans penis. He consulted an old physician of this city, now dead, who in a few days cured the clap, and then began to apply escarotics to the external sores, taking care, at the same time, to exhibit mercury. Shortly after this period a bubo in each groin made its appearance. That on the left side was repelled by leeches and cold applications; but the leeches refusing to sit on the other, it suppurated, burst, and discharged matter. Mercury was still continued to be given, and in such doses, that it at last brought on a severe ptyalism. During six weeks he remained in Glasgow under this treatment without amendment, but rather the reverse. The bubo became deep and spreading; the chancres did not heal, but increased in size; and, at the end of that period, had consumed a considerable portion of the under surface of the glans.

He now left Glasgow and went to Fifeshire, where he put himself under the care of a surgeon in Kinross. During a stay of four months here, the same plan of treatment was pursued. The constitution was kept under the influence of mercury, and the sores were dressed with dry lint. In spite of all this, however, the ulceration in the groin spread upwards and downwards, and that on the penis became more and more extensive.

Disheartened and alarmed at this state of things, he left Kinross, and removed to the Edinburgh Royal Infirmary. There the sores were poulticed; mercury was discontinued; tonics and a nourishing diet were used in its stead. Still, however, the disease was not arrested—the ulceration persisted in its ravages. On the penis, the whole frenum, with the lower segment of the glans, was destroyed; and the urethra was exposed down to the inferior insertion of the *frenum preputii.* In the groin, things looked no better; for here the phagedena extended downwards betwixt the upper part of the thigh and the scrotum, and along the perinæum, until it reached a point situate about an inch and a half from the anus. All this time the scrotum remained unaffected, and the discharge of urine along the urethra was quite free and unattended by pain. As might be expected, however, pain in the ulcerating parts was great, and the constitutional irritation proportionable. Now, mercury was again had recourse to. The mouth was made gently sore by its means; and after using it about a month, the sores began to improve. The improvement advanced slowly until the sore on the penis became cicatrised; and that in the groin and perinæum gave promise of being in due time in the same favourable state. The constitutional irritation was abated, and every thing looking well, when, without previous warning of such an event, and whilst he was attempting to make water, the urine one morning burst out suddenly from an opening in the perinæum, or rather in the angle formed by it and the upper part of the thigh, at a point rather less than half the distance betwixt the testicle of the right side and the extremity of the rectum. Since that time till lately, a period of nearly sixteen years, his urine continued to flow by this fistulous opening, and not a drop found its way by the natural passage.

Soon after the event which I have detailed, he was discharged from the Infirmary, and desired to retire to the country, in the hope that fresh air would be more conducive to the healing of what remained open of the sore in the perinæum, than the diseased atmosphere of the best regulated hospital. Here, the sore was soon cicatrised; the fistulous opening acquired a sound smooth skin; and his urine passed freely by it, without giving him the smallest pain or uneasiness. Twelve years after these events, a small abscess burst from the urethra immediately above the scrotum; but it healed up instantly without giving him farther trouble. This, he imagined, was occasioned by the irritation of a solution of the sugar of lead which he injected into the urethra on account of some heat and pain which he felt in that tube; but it is more likely that the heat and pain arose from the abscess forming, than from the very innocent means which he took to allay or remove them.

It may be proper, at this stage of our narrative, to mention, that our patient, soon after the ulceration had healed up, married a young woman to whom he had been affianced before his disaster; and that to the author he frequently declared, that he apprised her, in as delicate a way as possible, of his impotence previous to their nuptials; but that she refused to permit this circumstance to operate as a sufficient reason for retarding or preventing what had once been agreed upon. He likewise confessed, that he frequently attempted, after his marriage, to pay the conjugal debt; and that, though the seminal fluid was always discharged through the fistula, his sensations from the act were similar to what they were previous to his misfortune, and to what he supposed other men to experience whose organs remain as they came from the hand of Nature. But enough of this.

Sect. II. *State of Parts when the Patient applied to me.*

From the foregoing statements, the reader will, it is presumed, be at no loss to understand what was the exact state of this man's genitals when he applied to me, in the hope of having them restored to their natural state and functions. Externally, the lower segment of the glans was wanting;—the cicatrix of the ulcer which had been in the groin and perinæum was very manifest;—and the orifice of the fistula, with its glistening lining membrane, was seen in the situation which I have already marked. Internally, a bent probe or small bougie could be introduced by the fistula backwards into the bladder; but it was found impossible to direct such an instrument forwards towards the glans penis, on account of its being immediately opposed by the kind of *cul de sac* by which the anterior part of the urethra terminated. A catheter or bougie might be * introduced by the orifice of the urethra, and carried downwards to the obstructed part, which was situate from four to five inches down in the canal, and seemed to occupy, as near as I could judge, about an inch and a half of the tube. The patient is a little man, and, according to my most accurate ad-measurements and calculations, the obliteration (for it was not stricture) occupied the place of the bulb, and perhaps a small portion of the membranous part of the urethra.

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* It has been asserted, that the urethra, like the other mucous tubes of the living body, if its function be "for several years" suspended, will lose calibre, and ultimately be obliterated. But though the function of the fore part of this man's urethra had been completely suspended for sixteen years, this doctrine received from it no illustration. Vide page 89 of "An Essay on the Diseases of the Excreting parts of the Lachrimal Organs, by William Mackenzie. London, 1819." An elegant and useful little compendium.

It may well be asked how could so much as an inch and a half of the canal be obliterated in so short a space of time as one night, the period which, by the patient's account, intervened between the time when he transmitted his urine freely along the whole course of the urethra, and that when, refusing to be propelled forwards, it suddenly burst out by a fistulous opening in the perinæum? To this inquiry it may be answered, that it is by no means necessary to suppose, that the obliteration, to its whole ultimate extent, took place in the short period specified; or even that it had done more than barely commence when the patient made water that morning. We sufficiently account for the fact of the urine coming by the perinæum when we state, what undoubtedly was the case, that the ulceration had penetrated so deep as to destroy the continuity of the lower side of the urethra at a particular point; and that the urine, finding such breach or outlet, and likewise some resistance to the *nisus* which it made to go forward as before, took the readiest and freest route; and therefore came into day by the ulcerated opening in the perinæum. The subsequent steps of the obliterative process were probably the following. They consisted, I doubt not, *first*, of ulceration of the mucous membrane of the urethra to an extent equalling, if not exceeding, that portion of the tube which was ultimately found to be obliterated; *secondly*, of adhesion to one another of the sides of the tube, thus denuded of its mucous membrane, to an extent comprising the whole, or, at any rate, a considerable part of what was found afterwards to be obliterated; and, (if we suppose the obliteration not to have been *wholly* owing to the adhesive process after the phagedenic ulceration had ceased); *thirdly*, of granulation, or the formation of solid new flesh in the place of that which had been destroyed by ulceration, thus completing what was left undone by the adhesive process. When the urine first issued from the perinæum, perhaps neither adhesion nor granulation had done more than barely commence; at this period, probably, no more than simple destruction of the inferior side of the urethra at a particular point had taken place; and, it is more than probable, therefore, that had a bougie, or sound, or catheter been then introduced into the urethra, it would have met with little or no obstruction to its progress onwards to the bladder. But this very obvious expedient, so far as I have learnt, having been neglected, and no local means adopted to prevent the closure of the canal at the ulcerating point, adhesion went on, and granulation went on, until a firm and impenetrable obliteration took place at the point, and to the extent which I have already condescended on.

SECT. III. *Possible Modes of Treatment.*

Such was the condition of this individual when he consulted my late partner and me in November 1822. After we had examined it with much care, it was matter of grave debate whether the case ought to be meddled with at all; whether he ought not rather to submit patiently to the inconveniences which he had sustained so long, and become so much accustomed to, than endanger the perfect health and moderate comfort which he enjoyed by a doubtful, painful, and, at best, tedious attempt to redress his grievances; whether, in short, it would not, upon the whole, be wiser in him "to bear the ills he suffered than blindly rush on others which he knew not of." Impotence, in fact, was the only real evil which this man laboured under;—he could retain and discharge his urine with nearly as great facility as any man; with this difference only, that instead of presenting the pot-de-chambre before, he was obliged to place it between his thighs; there was no disagreeable discharge from the fistula, it was quite whole and presented as good an orifice as that of his urethra *behind* the glans;—his health, as has been said, was good; his sensations from the *coitus* were exquisite as other men's.

It was urged, on the other hand, that the patient was still far from being old, that he was willing to undergo almost any degree of suffering to be made like other men—that the resources of art were now numerous, and, wielded by skilful hands, were in modern times remarkably successful; that the case in some respects was perhaps unique, and presented a tempting opportunity of assaying the restorative powers of one or more of those agents, the advantages and dangers of which are still the subjects of much disputation; and, finally, that if *we* should be so deficient of courage as to recoil from the enterprise, the state of the patient's mind left us little reason to doubt that another of the profession would soon be applied to, who, more sanguine of success, and bolder in the application of his resources, would dare the hazards of the experiment, and fortune, generally propitious to the adventurous, might reward with success his manly and generous endeavours.

The latter train of reasoning prevailed, and it was resolved to commence forthwith some plan of treatment. It was doubtful, however, which of the methods at present in use among the profession of remedying states of parts similar or analogous to that which I had to encounter ought, in the present case, to be adopted. The disease was obliteration, not stricture. It possessed, no doubt, several features which allied it to the family of strictures, but still it had others, and these of considerable importance, which distinguished it from them. In stricture, an obstruction only of the canal exists, arising from an approximation of its sides to a smaller or greater extent, which again is caused by a morbid deposition of new matter upon, or within, that which originally formed the healthy parietes of the tube. Here, there was complete obliteration of a part of the urethra, not, as in stricture, from an accretion of new matter, but in consequence of a destruction of its original and healthy structure. Notwithstanding this marked discrepancy between the two cases, however, the points of resemblance were still numerous and close enough to determine us not to seek for any novelty in the nature of our purposed operation, but to content ourselves with using one or other of those agents whose powers in the treatment of stricture have been more or less illustrated by surgeons of reputation and experience. These are, first, the caustic bougie, as recommended by Sir Everard Home; second, the scalpel and catheter, as used by Mr Charles Bell and other surgeons in particular cases; and third, Dr Physick's species of bent trocar and canula.

In such a case as I had to deal with, it is obvious that the common bougie is useless, except as the auxiliary of other agents; for here a new passage was to be made, not an old one dilated.

An objection almost insuperable to the use of the scalpel, as employed by Mr Bell* in the case of Huggens, was, that the scrotum and testicles were suspended like a curtain over the very part which it was necessary to cut down upon. And besides this, I confess that the difficulties which this able surgeon experienced, and the dangers which his patient incurred, both during the operation and in the after treatment, would deter me from imitating his example in any case where another plan might be pursued, notwithstanding the great, though partial, success which at last rewarded his boldness and skill. This method, then, which consists of introducing and pushing a sound or catheter down to the obstructed part; of cutting down upon it and dividing the obstruction throughout its whole length; then, of pushing on the catheter to the bladder, retaining it there, and allowing the external wound to granulate and close over it, may, with propriety, perhaps, be adopted in some rare cases; but, for the reasons which I have stated, I did not think it a fitting operation in the one before us, and therefore, in consultation, I opposed its being adopted.

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* Treatise on the Diseases of the Urethra, &c. page 260.

Dr Physick's proposition I think well of, and imagine that it may be practised with signal advantage in impassable strictures of small extent, and where there is no choice, as often happens, except betwixt it and puncturing the bladder. In a case, however, such as N's, where the obstruction was of extraordinary length, and where the urine had abundant outlet by a fistulous opening, I conceive that a more gradual and perhaps less hazardous mode of restoring permeability to be the more commendable operation; and that the objection which Dessault urged against the caustic may be opposed with far more cogency to Dr P.'s plan. " On ne peut disconvenir," says he, " que ce moyen ne soit tres-ingenieux; mais qui garantira que ce 'trocar' * agira toujours dans la direction du canal, qu'il ne percera pas ce conduit, et ne formera pas de fausses routes ?" †

For these reasons, both the foregoing modes of proceeding being rejected, the only other which remained, viz. the use of armed bougies, was what was resolved on as being the most likely to combine the advantages, if not of ease and rapidity, at least of safety and success. But not wishing to take the whole responsibility upon myself, I was anxious that the case should be submitted to the inspection of my friend Dr King, who, as soon as he had examined it, and without knowing what had previously been determined on, at once said, with his usual acuteness and celerity of judgment, " This is an admirable case, Sir, for a trial of the caustic."

I am well aware of the antipathy which the French surgeons, with some exceptions, continue to entertain against the use of caustic in diseases of the urethra. I know too, that Dessault succeeded in terrifying his own imagination, and likewise attempted those of the profession, by calling this most useful substance " la pierre infernale;" but the authority of great names, as well as of ill names, when opposed to the dictates of experience, is, I trust, now abolished for ever; and the degree of experience which this great surgeon had acquired of the dangers of the caustic may be learnt from his own candid confession that " nous n'avons jamais osé faire usage de ce moyen; quoi-

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* " Caustique " apud Dessault. † Œuvres Chirurgicales, tome iii, page 246.

que ce praticien (Hunter) nous assure en avoir obtenu des succes qui ont surpassé ses esperances." With respect to caustic, therefore, the authority even of Dessault ought to have but little weight; the experience of Hunter and of Home is worth an infinite series of such authorities.

But even in Britain, opinions hostile to the use of caustic have often been promulgated; and if we may judge from the floating and unedited sentiments which are prevalent among the surgeons of this city, the application of this powerful agent to obstructions of the urethra must and ought to yield altogether to the employment of the various kinds of dilating instruments. I do not intend, however, at present, to enter into this important discussion; and will restrict myself to saying, that, so far as my experience extends, there is no agent with which I am acquainted, that in its real and positive effects upon the living body, so completely contradicts and belies all hypothetical and *a priori* reasonings respecting it as does the lunar caustic. Beforehand, we are apt to imagine, that when brought into contact with a chronic and indolent diseased mass, it will rouse such a part to a high and painful state of excitement, and if presented to an irritable and inflamed part, it will exalt such irritability and inflammation to their most unbearable and dangerous degree; whereas the truth is, that in both cases (I speak advisedly) although a sensation of heat or pain more or less smart is unquestionably felt at the moment of applying it, yet when the application *is continued long enough*, its mode and period of action are so exceedingly safe and mild as to justify me in calling it a sedative escarotic, and in soliciting for it a fair trial from all those who may be disposed to condemn untried so very valuable a resource in the treatment of urethral disease.

But there are other surgeons who, admitting the great utility of caustic in destroying "the firm and sharp stricture of small extent," condemn its use in "diseased narrowings of the canal, extending to half an inch or an inch in length, and irregular in their course;" who shudder at the idea "of boring with the armed bougie through the firm strictured part of the canal for so great a length, when the surrounding parts are comparatively loose." * Now I will not attempt to oppose these *dogmata* of the metropolis by any thing like hypothetical reasoning, but rather proceed to lay before the profession a detail of my manipulations with the caustic in N.'s case, in which the obstruction was much more extensive than the greatest which Mr Bell contemplates; and, in reference to caustic, if there be any analogy between stricture and obliteration, the complete success which ultimately crowned my exertions will, it is presumed, go some length to remove the prejudice which Mr. Bell's high authority must have excited against its employment in strictures of great length and firmness.*

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* Vide Mr Bell's Work formerly quoted, p. 133, 134.

SECT. IV. *Remedial measures which were adopted.*

The remainder of this paper is extracted almost verbatim from my note-book, written at irregular intervals during the progress of the cure, just as any thing remarkable, either with respect to fact or reflection, occurred to me. I might have revised the whole, and given the facts and observations which it contains a different order from what they at present assume; but I question much if I would not, by such an attempt, have curtailed much of their utility and interest. In the narrative of a case, I like to see every thing set down immediately after it has occurred, with the hopes and fears which the ever-varying aspect of affairs may have raised in the mind of the practitioner. In this way, we are admitted, not only into his surgery, but his mind; and as a fact or an idea set down at the moment of its occurrence is worth a cart-load of recollections, the result in point of instruction is infinitely to be preferred to the cold, sometimes glozed, and often constrained, subsequent lucubrations of the study.

1822. Nov. 15. After having explored the passage, by means of a common bougie, an armed one, well oiled, except at its very point, was slowly pushed down to the obstructed part, for the first time, to-day. It was kept applied during two minutes after a sense of burning pain was felt, and then withdrawn. The pain was inconsiderable at the time, and soon went off. On the second morning after applying the caustic, the patient, by squeezing the penis and bringing his finger and thumb forward to its extremity, forced out a small bit of slough and a little thickish dark-coloured pus.

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* When Dr King was consulted in this case, he grounded his opinion as to the propriety of using caustic for its cure on the success *he* met with in one of a somewhat similar nature which he had formerly treated. It was an impassable stricture of great length and firmness, complicated with much loss of substance, and fistula in perinæo. He employed caustic to reopen the passage, and the patient did well. None of the mischiefs which Mr Bell, and other surgeons dread, were experienced here. We, perhaps, therefore, do not generalize too fast, when we say that caustic, under proper restrictions, care and circumspection, may, with safety, always be employed in instances of *extensive stricture*, as well as of obliteration; certainly not, however, to the exclusion of dilating instruments in the great majority of cases. The entire safety of caustic in long and firm strictures, a point which is disputed, is all I contend for.

17. It was determined to allow a day to intervene between every application of the caustic and the next one. The same process was therefore gone through to-day, and with exactly the same results. There is little heat, and no swelling about the penis. The caustic gave very little pain, and even that was gone, or nearly so, before the patient left the surgery.

But really there is so great a sameness in the nature and complexion of my notes after this period till the 16th January 1823, that it would be tedious, and perhaps not very useful, to copy any more of those which relate to that period—a summary of them will perhaps answer every good purpose. Suffice it to say, then, that the armed bougie was introduced about thirty different times, that is, once every second day for a space of two months. Occasionally a small slough came away, loosened sometimes by the common bougie, upon the second day after the caustic had been applied—but this did not occur often —more generally the thick dark-coloured fluid formerly mentioned was all we observed as the effect of the caustic. The progress was rapid. I soon felt a roughness at the obstructed part; and, after a considerable number of applications of the caustic, there was a feeling communicated to my hand as if the point of the bougie made a kind of leap from the old beginning of the obstruction, through the new opened portion, to the part which was still obstructed. Besides these particulars, nothing remarkable occurred till one day (16th January 1823) the bougie evidently passed onwards to the bladder. It was then withdrawn, and a small silver catheter was attempted to be introduced; but this, from the terror of the patient, and the caution which I deemed it necessary to exercise, was found to be impossible. Dr King had requested to be informed of our progress from time to time. We therefore called on him. He likewise attempted to introduce a catheter into the bladder, but, for the same reasons which had restrained myself, he soon desisted. He, however, did not doubt that I had got through the obliterated portion.

January 17th.—To-day I succeeded in getting a catheter into the bladder, and urine was discharged by it for the first time during a period of sixteen years. The patient was overjoyed, and my satisfaction I will not take time to express. His urine was again drawn off in the evening by the same means, it being our intention to permit as little as may be to pass by the fistulous opening.

18th.—The patient complains of pain about the lately obstructed part; and there is a good deal of heat and some fulness in the perinæum. Notwithstanding these symptoms, I attempted to discharge his urine by means of the catheter, but failed. I was obliged to permit him to get rid of it by the fistula. I attribute my want of success to-day to some tumefaction or spasm which may have taken place about the parts in consequence of irritation. His bowels were opened by medicine, two grains of opium were afterwards given, and the perinæum fomented with a warm infusion of camomile flowers.

20th.—Pain abated. The urine flows in a free stream by the fistula. It is determined to permit as little urine as possible to come by it in future, so as, by suspending its function, to dispose it to contract, and, at the same time, to accustom the urine to the route of the urethra. For this purpose I attempted to introduce an elastic gum catheter into the bladder. I succeeded, and it was allowed to remain there the whole of this day. In the evening, when the patient strove to make water through it, he could not. It was therefore withdrawn, and it was discovered that the urine had been prevented from entering the instrument by reason of its *foramina* being plugged up by a sort of tough pus or mucus. The small silver catheter was then entered, and urine flowed by it in a free stream. It was kept in during the night of the 20th, and the two following days. When withdrawn, the silver was tinged of a black colour, but I observed not the least tendency to incrustation.

22d.—A larger gum-elastic catheter than the last was this evening inserted. The urine flows by it, and I was gratified even to find that a little came between the catheter and the sides of the urethra. Not a drop allowed to come by the fistula.

26th.—About this time a hard swelling was discovered in the line of the *corpus cavernosum penis* of the right side, over the ramus of the ischium. To-night it became painful, and the patient had rigors. The catheter was removed, and instantly about two or three ounces of pus, mixed with urine, the patient thinks, rushed out by the proper orifice of the urethra. The swelling immediately fell, and the hardness, in great measure, disappeared.—I am afraid of infiltration of urine into the cyst of the abscess, and therefore have directed the patient to discharge all his urine by the fistula; and, at the same time, by pressure on the perinæum, to prevent any part of it from getting past the internal orifice. Pressure by means of a pad and proper bandage has been applied over the supposed cyst of the abscess.

30th.—The hardness and swelling have disappeared, and no pus has come, since last report, by the urethra, or any other way which I could detect. To-day, in order to prevent the late obstructed part from reclosing, a middle-sized bougie was introduced. It readily passed the new-opened part, but would not, without violence, go on to the bladder. It was therefore taken out, and after it there appeared a very considerable flow of blood. Pressure on the perinæum, and cold applications to the genitals, were prescribed.

31st.—The bleeding continued rather profuse for three or four hours last night, and even persists in oozing a little today. Cold applications and pressure to be continued. It is evident that my efforts to pass the bougie yesterday had been persisted in too long, or that the bougie itself had been rather large.—If any thing more than another is to be learnt from the occurrence of the hemorrhage, it is the necessity of extreme caution and delicacy in the employment of instruments about the urethra.

Feb. 3d.—Since last report, no attempt has been made to introduce a catheter or any thing else into the urethra, in order to allow time for the ruptured vessel to heal. The patient makes water by the fistula. The bleeding ceased on the 1st instant. To-day, I attempted to introduce a bougie, and afterwards a small catheter. It was found that the new-made opening through the old obstruction was of less calibre now than before the hemorrhage; for instruments of a size which passed readily then, were pushed on with difficulty to-day. As the pain attending their introduction, however, is not greater, but less, I attribute the increased difficulty of passing them to the contraction which attends cicatrisation.—By the daily use of a bougie, I hope to remove this contraction, and to get a catheter of the same size as formerly into the bladder. No more bleeding.

7th.—The urine passes freely, and without pain, through the fistulous opening. The small silver catheter, which formerly passed readily into the bladder, cannot be got, without violence, to pass the old obstructed part; but bougies of a good size, and a pretty large gum-elastic catheter (indeed one thicker than the metallic one), pass it with little difficulty, but cannot, without so much force as would cause much pain and perhaps hemorrhage, be pushed on to the bladder. They seem to stick somewhere about the prostate gland. The patient is now very timorous, and complains when even a safe degree of force is employing. No blood to-day.—I am determined to be cautious;—to introduce a bougie every day as far as I can without violence, in order to dilate the new-made portion of the urethra;—to keep steady pressure applied over the cyst of the abscess;—and to allow all the urine to pass by the fistula, until the urethra has so far recovered from the late accidents as to warrant a serious attempt to pass a catheter on to the bladder.

8th–26th.—For some days we suffered great discouragement. I could not get a bougie of the smallest size to enter the bladder. The small silver catheter was stopped at the seat of the old obstruction; and, when pushed with gentle force, it seemed to enter the opening into the cyst of the abscess, and its curve and point were felt out of the tract of the urethra altogether, towards the right side, within the cavity of the abscess. Little hope now remained of success. The patient's spirits sunk, and all our anticipations of a happy cure seemed to be defeated. Next day, however, the patient's courage rallied, and a new attempt to reopen the obstructed passage was determined on. Accordingly, an armed bougie was introduced, and pushed down to the obstructed part, the patient being directed to press with considerable force upon the point where I conceived the opening into the abscess to be, in order that the point of the bougie might not, as the catheter had done, enter it. For the usual space of two minutes, it was held applied to the obstructed part, without producing a sense of burning, and then withdrawn. Next day (16th), there was a considerable discharge from the orifice of the urethra. The silver catheter was introduced, in order to discover what progress had been made by the caustic in destroying the young adhesions. The place where I suspected the opening of the abscess to be was firmly pressed upon, and gentle force was used in pushing on the catheter; the consequence of which was, that the instrument went onwards, through all obstructions, to the bladder, and urine, to our great satisfaction, was discharged through it. Having thus happily succeeded in getting the catheter once more into the bladder, it was judged imprudent for some days to withdraw it, so that its pressure in the urethra might completely reestablish the natural passage. It was accordingly fixed in the bladder by the usual means, so that it could not slip out, and allowed to remain there during the space of five days. The urine came freely through it, and none came by the fistulous opening. The catheter was, at the end of this period, withdrawn to be cleansed. The patient felt no inconvenience from its presence, but I was afraid of incrustation upon its point; however, there was none. A gum-elastic catheter was judged to be a safer instrument for our present purpose, and one of that kind was substituted for the metallic one. It passed with the greatest ease into the bladder, and was allowed to remain there for other five days. The discharge of mucus from the urethra was at this period considerable, but no appearance of pus, as before, was observed. Pressure was kept up over the abscess. The swelling and hardness in this part are almost entirely gone. When the catheter was withdrawn to be cleansed, the patient attempted to make water by the urethra, and succeeded. He at the same time made firm pressure upon the fistulous opening, otherwise part of it would have come that way. It passed with considerable force and fulness of stream. This experiment, however gratifying, was not often made, as there was still some danger of infiltration into the cyst of the abscess. No pain was felt when the urine was passing by the urethra, which led me to believe that great progress had been already made in the healing of the burnt sides of the canal.

26th.—Every thing appears to be doing well. The patient himself can take out and put in the catheter with perfect ease. The discharge of matter, perhaps of a muco-purulent nature, is considerable. This I attribute to the irritation kept up in the canal by the constant presence of the catheter. I have thoughts of soon giving up the use of the catheter altogether, as the sides of the urethra seem to be quite whole. The employment of common bougies, to keep the passage of its present width, or to dilate it if necessary, will, perhaps, suffice to complete the cure. The urine, I think, might now be allowed to pass wholly, without a catheter, through the urethra; there being no symptom of infiltration when it is permitted to do so, and there is no pain.

March 4th.—Since last report, the urine has regularly, with very few exceptions, been drawn off by means of the gum-elastic catheter, which continues to pass into the bladder with perfect freedom. A pretty large-sized bougie has been introduced two or three times every day, in order to widen the urethra at the old obstructed part. The discharge is very much lessened since we ceased to keep the catheter constantly in the urethra. The patient is out of doors daily. Pressure by means of the pad has been kept up over the cyst. Notwithstanding this, however, that part does not yet appear to be whole; for, this morning the patient discovered a small globule of pus or mucus escaping by a very minute orifice in the perinæum, at the point where the hardness has always existed. When I attempted, by means of a probe, to enter the cyst, I found it difficult to accomplish, and therefore desisted. It appears, that, by way of experiment, he lately attempted coitus with his wife; and, what is to me at present unaccountable, not a drop of semen issued by the urethra, all of it having escaped by the fistulous opening in the perinæum. During the act, he applied pressure by his hand over the orifice of the fistula. Pressure applied here prevents urine from getting out that way, and directs it along the proper route, but it has not the same effect with semen.

7th.—All discharge from the urethra has ceased. Pain while making water without the catheter, gone. No discharge from the cyst, either into the urethra, or from the perinæum. The hardness in the perinæum lessening. The only unpleasant fact to be noticed to-day is, that little or no urine will flow by the urethra, without a catheter, unless a bougie be passed immediately before making the attempt: then, but not till then, the water issues in a full and free stream. This proves, either that a morbid, perhaps spasmodic, collapse of the part recently made open, takes place soon after the bougie or catheter is withdrawn; or, that there is a want of consent between the muscles about the neck of the bladder and urethra, which, according to their several functions, retain or expel the urine. The former of these is the more probable cause of the phenomenon; because the urine, upon these occasions, though it will not pass along the whole course of the urethra, flows freely enough by the fistulous opening.

June 8th.—The case has gone on as well as could be wished since the date of last report. As I have already mentioned, the only disagreeable circumstance was, that till very lately, for about two or three hours every forenoon, the urine refused to proceed by the natural passage, unless a bougie was first introduced to the bladder. During the night, morning, and afternoon, it issued freely without the aid of such an instrument. This symptom, I became convinced, arose from a spasmodic contraction of the sides of the recently opened portion of the urethra. To take off the morbid sensibility of the part, which probably caused the spasm, I ordered an armed bougie to be introduced down to the part every day, and to be kept applied to it for about a minute. This operation the patient performed for himself, and the happiest effects ensued. Little pain was felt at the time; but a considerable discharge appeared afterwards. At this date, and for some days past, he can discharge his urine freely, by the natural passage, at all times of the day, without more than the usual degree of pressure. The stream is strong and copious. He likewise has regained the power of transmitting the seminal fluid along the whole length of the natural canal. In doing this, however, he is still obliged to press with considerable force upon the orifice of the fistula.

The case is now reduced to a state, resembling in most points, those instances of unnatural apertures in the urethra, which have been so ably treated, first by Sir Astley Cooper, * and then by Mr Earl, † upon the principle of transposition. In any attempt which may be made to close the fistula, much light and direction will be afforded me by invaluable cases of these gentlemen ; but I may be induced, from its peculiar form and size, to proceed upon a somewhat different plan, at least in the first instance. The opening is so very narrow, that I think an instrument may be constructed upon the principle of a cheese *pale*, as it is called, or of a wright's *wimble ;* which, being introduced into the fistula, and turned round, first the one way and then the other, will, I imagine, so denude its sides of skin, as to make their adhering to one another, when pressure is applied, almost a matter of certainty. Should this plan fail, however, the actual cautery, now so much employed by the French, or the nitric acid, as used by Sir Astley Cooper, will, I think, infallibly succeed ; and if all these resources disappoint us, we have still the Taliacotian method in reserve.

15th.—Two days ago, I understand, my patient found it necessary to leave town, under circumstances which exclude all hope of his return to his wife and friends; but which prove, in the most conclusive way, the completeness of the cure, and his perfect confidence in his restored virility.

The details of this not unimportant case, have already been drawn out to such a length, that I have left myself no space to indulge in those general reflections which naturally arise out of its history; but this is the less to be regretted, as almost every thing of a practical nature has been at least glanced at, as we advanced.

Glasgow, 6th February, 1824.

XV.

Case of Sloughing of the Bladder, consequent on Parturition, successfully treated, with Remarks. By JAMES GUTHRIE, Surgeon, Kilmarnock.

On the 21st May, AGNES CAMPBELL, aged 23, after having been three days in a severe first labour, was delivered by the forceps of a dead child. In the early stage of labour, she voided her urine frequently, but, towards the end, a total retention took place. An attempt was then made to introduce the catheter, but without success. Immediately afterwards, delivery was accomplished by the forceps. The exact time that the head of the child rested on the perinæum, I cannot determine, but it was considerable. As I waited six hours after being called, before instrumental aid was given, she suffered very little from the application of the instruments, and, after delivery, she felt tolerably well.

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* Surgical Essays, Part II. p. 207, published in 1819.

† Philosophical Transactions, Part II. for 1821.

Next day (22d) when I visited her, she complained of an excessive discharge from the vagina; great pain and tenderness of external parts. She had discharged her urine about an hour after delivery, since which time she had no power of retaining it. This patient has a highly offensive smell, and the discharge is so great as to make its way through the bed, notwithstanding the repeated application of flannel cloths. Castor oil was ordered, and the external parts to be bathed with tepid milk and water.

23d.—Complains greatly of pain in belly, particularly in the hypogastric region; urine not retained; external parts much swelled and inflamed; pulse quick; skin hot; thirst great; had a shivering fit last night; belly very tender when pressed, and is almost as large as before delivery.

Venesectio ad ℥xvi. S. Calomel gr. x. et post hor. quat. Sulph. magnes. ℥i. Applicet. abdom. Ol. terebinth.

24th.—Blood cupped and buffy; calomel and salts operated six times; feels somewhat better; complains still of pain in belly; pulse not so rapid; incontinence of urine; drinks incessantly.

Rep. V. S. ad ℥xij. S. statim Calomel gr. x. Contin. Ol. terebinth.

25th.—Blood last taken not so buffy; calomel operated; feels remarkably well; no pain in belly. Pulse natural; incontinence of urine still continues; external parts very tender and painful; fetid discharge from the vagina; sloughing is making rapid progress. Tepid milk and water to be frequently injected into the vagina, and great attention paid to cleanliness.

28th.—Has attempted to rise for the first time; and on getting into the erect posture, a large quantity of urine flowed from the vagina, producing excessive pain and irritation. The urine, she says, dribbles constantly from her when in bed, which renders her situation truly miserable and uncomfortable. This patient thinks her state so desperate and irremediable, that she is calling out for some person to end her existence.

These symptoms induced me to suspect injury of the bladder. I accordingly examined her; and having introduced a finger into the vagina, and a catheter into the bladder, I discovered an opening large enough to allow two fingers to pass into the bladder, and through which the catheter was felt exposed for more than an inch. The situation of the aperture was directly about, or rather above the *cervix vesicæ.* Its edges felt soft and irregular; she complained much of pain during the examination.

Being now satisfied, that a communication by sloughing had taken place between the bladder and vagina, I proceeded, as soon as the tenderness of the parts would permit, to adopt the following plan of treatment; although I must state, on the very threshold, that I despaired of being able to render this wretched patient any assistance.

June 2d.—A piece of sponge was introduced into the vagina, and applied in direct contact with the perforation in the bladder, to which was attached a ligature, for the purpose of withdrawing it when found necessary.

A short elastic gum catheter was next passed into the urethra, which being fixed by suitable bandages, was allowed constantly to remain, in order to prevent the distention of the bladder, and to keep open a free passage for the discharge of the urine, as it collected in the bladder. In this way, the edges of the aperture were brought into more immediate approximation.

June 5th.—Urine flows entirely by catheter; sponge completely prevents it from passing into the vagina; feels much more comfortable; considerable discharge of offensive matter from vagina. A solution of *acet. plumb.* was ordered to be injected frequently into the vagina.

7th.—Excessive discharge of fetid matter from vagina; complains much of pain and tenderness of external parts, which are now in a state of ulceration; urethra very irritable. Sponge and catheter withdrawn, cleaned, and again introduced. Solution continued.

10th.—Tenderness and pain so great, that both sponge and catheter were withdrawn without my knowledge. After this imprudent act, she says that she retained her urine for three hours, and discharged the greater part of it by the urethra, which was attended with excruciating pain. She was at this time examined; and having introduced a finger into the vagina, I was astonished to find the aperture so much contracted, as scarcely to admit the point of the finger; tumefaction of the parts greatly diminished. She does not complain of so much pain during the examination as formerly.

This patient is very averse to submit again to the application of the remedies; her sufferings, she says, under their use, are almost unsupportable. However, by holding out to her the prospect of ultimate relief, and the certainty of a complete cure being obtained, she came to the resolution of giving them another trial. The sponge was again introduced in the same manner as formerly stated, and allowed to remain.

The catheter was likewise passed into the urethra, and steadily fixed by a bandage similar to that used in diseases of the perinæum, A ribbon was tied to the extremity of the catheter, and both ends of it were then fixed to a transverse bandage previously put round the belly. This simple contrivance answered completely my expectations; indeed, I had no other in my power to adopt. The urine was collected at the end of the instrument. The catheter, it will be understood, was left constantly in the urethra. At the interval of every three days, the sponge and catheter were both withdrawn for the purpose of being cleaned, after which they were immediately introduced. The comfort afforded by these means was evidently great. She was kept perfectly dry, and was enabled to walk about without inconvenience.

It will be unnecessary to trespass on the limits of your Journal, in describing the stages of amendment. Suffice it to state, that the above treatment was continued with little variation for the space of a month. At the end of which time, the aperture in the bladder was completely shut up, by a soft, but pretty firm cicatrix. There remained some degree of depression in the seat of the injury; but the communication betwixt the bladder and vagina was entirely obliterated. She now can retain her urine at pleasure, and is exceedingly happy that she has been freed from a state of misery, which, at one time, she never expected to be. The remedies were immediately discontinued.

I examined this patient five months afterwards. She still remains perfectly free of her complaint. There is a slight depression of an oval shape in the situation of the injury. Vagina is considerably contracted. However, she has not the least inconvenience in retaining or discharging her urine.

Remarks.—Perforation of the bladder, by sloughing, consequent in parturition, is, I am convinced, invariably the consequence of pressure, complicated more or less, with a distended state of the bladder. It may, no doubt, in some instances, but not frequently, be occasioned by the rash and improper use of instruments. In the present case, the instruments had no concern in its production. It was evidently caused by the continued pressure of the child's head over the distended bladder.

It is, I think, perfectly apparent, from the successful termination of the above case, that injury of the bladder, succeeding parturition, is generally curable when early and properly treated. The early adoption of the means of cure, is a point of so much importance, that I cannot speak too highly of it. The treatment necessary to be pursued is exceedingly simple. It consists, first, of a sponge placed in immediate contact with the aperture in the bladder; and, lastly, of a catheter kept constantly in the urethra. Having stated elsewhere their *modus operandi*, it will be needless in this place again to repeat it.

The catheter is the principal remedy, and must be allowed to remain night and day in the urethra, for the obvious purpose of keeping the bladder in the greatest state of contraction; which, I am satisfied, is the *sine qua non* of treatment. The application of the sponge to the aperture must on no account be neglected. The apertures at the end of the instrument should be large.

The rapidity of the cure of this unfortunate patient, after such extreme suffering, was very remarkable. The happy termination of the case exceeded my most sanguine expectations. The closure of the aperture may be said to have taken place about the end of the third week, as no urine at that time was observed to escape by the vagina. But, being afraid of fresh laceration taking place, from any accidental cause, I thought it advisable to continue the application of the remedies two weeks longer.

She endured for a considerable time the most excruciating pain, from the use of the remedies. However, her sufferings gradually became less, as the irritability of the parts subsided.

The management of this case differs in some respects from the one successfully treated by Mr Barnes, and published in 6th vol. Medico-Chirurgical Transactions.

As this complaint has sometimes not been recognised, or passed over in silence, without any method of treatment being attempted, I must beg leave to call the attention of the profession to the irremediable consequence resulting from neglecting examination, where a tender state of the parts and incontinence of urine exist, after tedious or instrumental labour. It can do no harm, and it may do much good. In fact, I would hold a practitioner criminally guilty, who would not avail himself of the evidence necessary to ascertain the existence of this dreadful malady; and, when discovered, it is of the utmost consequence, if we value the comfort of a human being, that no time be lost in having immediate recourse to the remedies pointed out in the treatment of the above case, at least as soon as the tenderness of the parts will permit. For, when neglected at the commencement, or not treated with sufficient energy and perseverance, I believe the case may be set down as incurable, and entirely beyond the reach of art. In the chronic state, it has been recommended, to make the edges of the aperture raw by touching them with caustic; but I believe this method has not been found successful. The application of issue ointment *(ung. pulv. canth.)* to the edges of the fistulous aperture, would appear to me preferable to the caustic. Whatever method is adopted, the sponge and catheter are indispensable.

Kilmarnock, November 18th, 1823.

XVI.

Further Remarks on the Treatment of Hydrophobia, by Injection of Narcotics into the Vein. By Richard Pearson, M.D.

I am induced to offer some remarks on the treatment of hydrophobia, supplementary to those inserted, three years ago, in the 62d Number (vol. xvi.) of the Edinburgh Medical and Surgical Journal.

In accordance with my wishes, those remarks, in which I suggested the probable utility of injecting medicinal substances into the veins in cases of hydrophobia, to subdue the spasms which prevent deglutition, have attracted the attention of physicians, both at home and abroad: And in a case which recently occurred in the Hotel Dieu at Paris, Dr Magendie speedily removed the spasmodic affection, and enabled the patient to swallow liquids, by injecting warm water into a vein of the arm. The convulsive movements and delirium ceased, and there was reason to hope a cure would have been effected. But the person subjected to this treatment appears to have been previously in a very unhealthy condition, and to have sunk under a complication of disease.

Notwithstanding the unsuccessful issue of this case, a most important fact has been established, viz. that by injecting warm water alone into a vein, convulsions and delirium, in the human subject, may be promptly subdued. How much warm water, or how much of any other liquid, should be employed for this purpose, is a question which remains yet to be decided.

To me it appears, that Dr Magendie erred in regard to quantity, when he injected two pints (French measure.) So large a quantity of fluid, suddenly thrown into the circulating system, could not fail to produce an over-distended state of the blood-vessels; and to this circumstance I am induced to ascribe "the hæmorrhage from the intestines" (which occurred on the second day after the operation);—"the distension of the veins of the small intestines (discovered on opening the body after death);"—"the reddish serum that was seen at the base of the cranium;—the fulness of the veins of the brain, and of the rhachidian prolongation" (spinal marrow); and some other appearances on dissection.

Nor is the production of plethora the only objection to so large a quantity of water being suddenly introduced into the circulating system. So much warm water, immediately mixed with the blood, must promote its tendency to decomposition, so observable in the last stage of this disease, and so conspicuous shortly after death. *

Therefore, *instead of warm water alone, and in large quantities*, I would still propose, as I did propose in my former communication on this subject, *warm water in very small quantities, and impregnated with narcotic substances*, † *to be injected into a vein in cases of hydrophobia*, for the purpose of subduing the spasms, and enabling the patient to swallow both liquids and solids. This grand object being attained, what is *then* to be done? I would propose the administration of a brisk cathartic, copious dilution with mucilaginous liquids, camphor and other antispasmodics; and, after the operation of these medicines, sponging the body with cold water and vinegar, and giving at the same time, internally, the nitric or muriatic acids; and, lastly, some of the vegetable tonics.

Sutton Coldfield, near Birmingham,
February 5th, 1824.

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* The rapid putrefaction of the bodies of persons who die of hydrophobia did not escape the notice of Morgagni, Sauvages, and others, as mentioned in my small tract on this disease, published more than twenty-five years ago.

† A state the very opposite to that of convulsion, a state of quiescence or collapse, speedily supervened in the two dogs operated upon by Mr Alfred Jukes, surgeon to the Birmingham Hospital, in the presence of Dr Booth and myself. (See Edin. Medical and Surgical Journal, Number 66. vol. xvii.) In one of those dogs, four grains of opium, dissolved in not more than ʒii. of warm water; in the other, not more than ʒii. of an aqueous infusion of digitalis, sufficed to produce the above-mentioned effect. Perhaps I was over-cautious in limiting the quantity of the aqueous vehicle to ʒii. it being possible that some of the fluid may be lost in the operation; but, at all events, I am of opinion that the quantity of water, impregnated with medicinal substances, and employed in this manner, should not exceed an ounce.

XVII.

A Case of Bronchocele, successfully treated by Iodine. By E. Barlow, M. D. Bath.

A female, 28 years old, delicate, but not unhealthy, applied to me early in November 1823, on account of a bronchocele of three years standing. On its first appearance it had been leeched, with the effect of softening it, and somewhat reducing its size, which was that of about half a small orange. It remained stationary for a long time, causing little inconvenience; but about two months previously to my seeing her, it began to enlarge, and increased so rapidly as to reach the size of above half a pretty large orange. At this time its pressure on the larynx and trachea sensibly obstructed respiration, and even impeded deglutition, while her general health had begun to decline. Induced by her former experience, I reapplied leeches; and following the practice which I had myself found most successful in relieving this complaint, I prescribed occasional purges of rhubarb and calomel, with the daily use of bark and burnt sponge. The latter she was soon obliged to relinquish, from its disagreeing with the stomach; and I willingly laid it aside, in consequence of a slight pectoral complaint, attended with febrile symptoms, and resulting from recent cold, which called for different treatment. Saline medicines, with squill, were resorted to, the aperients continued, and leeches were again applied to the tumour. Under this regimen her pectoral complaints were quickly relieved—her general health improved—the pulse was 96, and soft—the tongue clean, but whitish, and the bowels regular. The menses had been regular throughout.

On the 6th December I began the use of iodine, giving twice a day 6 drops of a solution of 36 grains of hydriodate of potass in an ounce of diluted water. This was progressively increased to thrice a day, then to 8, 10, 12, and 15 drops, which latter dose I did not for some time exceed, as the medicine evinced considerable effect in reducing the tumour. The leech bites having inflamed, I was obliged to poultice them, which prevented me using the iodine externally for some time. They healed, however, and, on the 20th December, I commenced with the outward employment of iodine, directing the size of a nutmeg to be rubbed in night and morning of an ointment consisting of a scruple of hydriodate of potass to an ounce of lard.

In a week this produced so much irritation of the skin, that one of the daily frictions was omitted.

Under this discipline the tumour rapidly subsided; no disturbance was occasioned in the stomach; the general health improved, so as to be better than she had known it for years; and, on the 10th January, scarcely a vestige of tumour remained. On the 17th the improvement continued, and only a thin film could be felt by the finger. I have since seen her in perfect health, and free from complaint. The rhubarb, with calomel, was occasionally employed throughout; and, for a short period towards the close, I increased the drops to 18.

This case seems to afford satisfactory evidence of the specific powers of iodine in reducing bronchocele. The disease appears not to be indigenous in this place; for in seventeen years I do not recollect having met with more than half a dozen cases in a tolerably wide range of hospital practice. These I have uniformly relieved by purging, and the use of bark and burnt sponge; but the progress has always been slow; and, in one instance, a very troublesome affection of stomach ensued, which a remark of Coindet * leads me now to attribute to the long continued use of the burnt sponge.

In administering iodine, it appears to me that precaution is necessary, *first*, in improving the general health, so as to afford the remedy fair scope for exercising its specific agency, a remark indeed which applies to the use of all specifics; and, *secondly*, in gradually increasing the dose, so as not to exceed the *minimum* quantity capable of acting on the local disease. With this limitation, I apprehend that this active remedy may be safely administered, without hazard of those injurious consequences which result from a too free or unguarded employment of it. Its efficacy in reducing glandular enlargement would lead me to think favourably of it as an auxiliary remedy for *tabes mesenterica.*

Bath, February 15th, 1824.

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* Coindet, Observations on Iodine, p. 8.

PART II.

CRITICAL ANALYSIS.

I.

An Account of the Yellow Fever as it prevailed in the City of New-York in the Summer and Autumn of 1822. By PETER S. TOWNSEND, M. D., Honorary Member of the Royal Jennerian Society of London; Fellow of the Literary and Philosophical and of the Physico-Medical Societies of New-York; Member of the American Geological Society, &c. New-York, 1823.

THE question of the origin and contagious nature of yellow fever is not yet decided; and, if we may judge from the number and character of the works which continue to appear on this subject, we may safely assert, that it is just as near being determined at the present hour, as at the time when the disease was observed by Moseley, Rush, and Hunter, and when the question of its origin was agitated by Hosack and Chisholm on the one side, and by Miller, Rush and Bancroft, on the other. Without entering into the merits of the assertion contained in the very beginning of the work before us, regarding the source and channel through which the disease was introduced, we find that yellow fever made its appearance, in July 1822, in the foot of Rector Street, New-York, and thence proceeded, at various rates, to prevail in other parts of the city during the summer and autumnal months, after which it subsided gradually, and finally disappeared, in the ordinary manner, about the beginning of November, when the frosts of winter commenced. In the present volume, which contains 277 octavo pages, divided into twelve chapters, Dr Townsend describes the mode in which it was introduced and propagated, the general symptoms, pathognomonic symptoms, diagnostic marks, autoptic appearances, pathology and treatment, in the order specified in the first seven chapters. In the 8th and 9th, he considers the subjects of prevention and purification—immunity and predisposition; in the 10th, the state of the weather previous to and during the prevalence of the epidemic, with numerous meteorological observations; in the 11th, the collateral diseases, which prevailed to a greater or less extent during the same time; and the 12th chapter, which the author justly terms the conclusion, consists of thirty-one aphoristic propositions or inferences which he believes he has deduced, according to the legitimate conditions of reasoning, from the phenomena recorded in the previous part of his work.

Each of these subjects Dr Townsend treats at considerable length, and with much care; but it is obvious that he possesses more of the character of the mere doctor than that of the close observer, the scientific physician, or the logical thinker. The arguments of which his reasonings consist are not always well chosen, or very conclusive;—he assumes rather than demonstrates the proposition which it is his object to establish, and he deals from beginning to end almost in a train of general assertions, to which his particular instances or facts are not unfrequently appended rather than adduced. His nosographical details are correct, and not too long; and for this he must receive the small portion of credit which accrues to all, who have described a disease which has been already observed and described not only by Towne, Warren, and Hillary, but by Moseley, Hunter, Rush and Jackson, and in which the only merit is to realize their accurate histories. We doubt whether his pathology of the disease truly deserve the name. His treatment is not exactly what we have been accustomed, by competent authorities, to regard as the best and most efficacious; but perhaps fashion influences this as well as other affairs of men and physicians. The general style of the work is in the worst possible taste for a medical treatise—declamatory where it ought to be argumentative, decorating, what ought to be mere narrative, with the flowers of a meretricious and false eloquence, and embellished with many of those strange idioms and unsanctioned expressions which American authors are too fond of substituting for plain English. Not satisfied with a novel and eccentric mode of constructing his sentences, Dr Townsend hesitates not to manufacture words, to express ideas for which we had thought there were already a sufficient number in use. Take, for example, the following sentence, which is one of the first that occurs to us:

'The mortability in the high and spacious avenue of Broadway, as much admired as the Boulevards of Paris or the Strada Toledo of Naples for its beauty, cleanliness and superb buildings, by all the foreigners who come here, like the street called the New-Street at Barcelona during the last year, teaches us what devastation the pestilence might have caused, had not as many persons deserted this part of the city as did.' pp. 67–68.

That we may not, however, incur the charge of partial examination of Dr Townsend's treatise,—of censuring in general and vague terms, what ought to be judged in detail, or of underrating the matter for faults connected with the manner of his book, we shall enable our readers to form their own opinion.

The chief characteristic of the present work is the zealous and determined manner in which its author espouses the side of contagion as the origin of yellow fever, and contends for its being, in all cases, an imported disease. In establishing this point, the author employs no fewer than 126 pages, partly facts, partly reasoning, partly declamatory assertion, which, in truth, is very nearly the half of the volume. But though this appears to be done in a most elaborate and detailed manner, the whole of the evidence on which his conclusions are founded, when divested of their adventitious matter, are contained in a small compass indeed, and may, without injury to the actual strength of the arguments adduced, be condensed within a much narrower space. The following quotations will not only prove this assertion, but will further show the kind of reasoning which the author employs, and the hardy, gay-faced manner in which he assumes the very point which he ought to prove.

"The yellow fever was introduced this year, 1822, into the city of New-York, by several vessels from Havanna. The following is the mode in which we have every reason to believe the contagion was brought to the wharf, at the foot of Rector-street, where the disease first made its appearance.

"Between the 25th day of June, and 1st day of July, 1822, there arrived at Quarantine, after a few days passage from Havanna, the following vessels:

The Brig Spanish Soldier, arrived	June 25th.
Brig Abeona, - -	28th.
Brig Ambuscade, - -	28th.
Ship Eliza Jane, -	July 1st.

"The *Abeona*, was what might be termed a regular trader to and from Havanna. The crew also, being all Spaniards, were therefore accustomed to the climate of the tropics.

"The *Ambuscade*, might be considered in the same light. Her captain was a veteran in the West India trade.

" The *Spanish Soldier lost two men with yellow fever on her voyage, one of whom died a few days before her arrival.*

" The *Eliza Jane*, also, lost *two men of yellow fever* on the voyage.

" The cargoes of all these four vessels, consisted chiefly of Havanna sugar in boxes, and it was all brought up to town, and landed at or near the foot of Rector-street, between the 1st and 9th days of July. The vessels employed to do this service, were *decked* lighters, having of course holds, where the air might be confined, and carried from one place to another, without the chance of being dispersed. Each lighter had from two to three men, all of whom had been in that service many years, and were frequently exposed, and necessarily habituated to the air of infected vessels from West India ports.

" Between the dates above specified, viz. from the 1st to the 9th of July, 24 lighter loads of boxes of sugar, out of the above named vessels, were brought up from Quarantine, seven miles distant from the city, and discharged at or near the foot of Rector-street, in the short distance between Carlisle-street, and the piers at, next to, and south of Rector-street. About five lighter loads were discharged each day, amounting to nearly one half the cargo of either of the vessels. More than one half probably of the cargoes of the above four vessels was discharged within three or four days of the period mentioned. The cargoes were stored in the ware-houses which stand and open on the wharves to which they were brought." pp. 23, 24.

" There never, perhaps, had accumulated before so large a mass of cargo from Havanna vessels, at that particular season, and in so small a space. One of them contained near 600 boxes." p. 25.

During the time the lighters were receiving their loads from the Havanna vessels, the thermometer ranged steadily at a high temperature (80° average at 2 *p. m.*, 78° medium for the whole day), the weather was unusually calm, and the air undisturbed by a breath of wind.

" The disease began at the two opposite corners of Rector-street where it terminates on the wharf in Washington-street, July 10th, 1822. As Washington-street has but one row of buildings, fronting the wharf, Rector-street, terminating also here, has but two corners. The N. W. corner was the cooper-shop of a Mr Reder, and his house, a neat two-story building, stood next to the shop on the same lot, but in Rector-street. The opposite corner was a building occupied as a grocery, by a Mr Falkner. The first cases were Reder's two little girls, the one named Amanda, aged 11, the other named Caroline, aged 9, and Andrew Thomas, a young Scotchman, aged 23, of a robust, hale constitution, who had only been in this country *seven weeks*, and who was clerk in Mr Falkner's grocery. They all sickened July 10th and 11th, 1822. Dr Waters was called to see Reder's two daughters the following day, July 11th. Thomas was sent immediately to the New-York hospital, without having been attended by any physician, and died there July 16th, with black vomit, but was not reported to the board of health, nor recognized in that institution, according to the best of my information, as a case of yellow fever." pp. 26, 27.

Dr Townsend having thus connected the two facts of arrival of Havanna vessels, and appearance of febrile symptoms in the inhabitants of Rector Street, proceeds to determine the origin of the sickness, which he does in the following summary manner.

"The great question now to be determined, is from what source these first cases derived their sickness. It is very evident, that both Thomas and the little girls, by falling sick within thirty-six hours of each other, with the same disease, in two opposite though different houses, derived their illness from the same cause. At least it is hardly possible to suppose, that a coincidence so extraordinary as this should have happened by mere chance. It is also beyond all question, that this was the beginning of the pestilence; for a few days afterwards an alarming number of persons took sick of the very same disease, most of whom died, in the houses in Rector-street, next to Reder's, and to the grocery. We see that the lighters had been discharging their cargoes at, above, and below the wharf directly at the foot of Rector-street, and not more than fifty feet distant from the houses where the three first cases happened, up to the day before that on which the Reders sickened. This alone is sufficient to account for the origin of the disease, supposing the Reders and Thomas to have had no direct communication with the lighters. But in addition to this, we know it to be a fact, that the two Reder girls, at least, were still nearer than this to what we consider to have been the source of the disease. Reder, in his vocation of cooper, was constantly employed along this wharf, in repairing water casks, boxes of sugar, &c., and was at that very time engaged in this business. This much is certain, that the little girls frequently visited their father while he was at work, and were in the daily habit of amusing themselves in playing about the dock. Why others did not fall sick until after the Reders and Thomas, and not at the same time with them, though many were doubtless exposed to the same original cause, cannot be determined. Because perhaps Thomas especially was peculiarly predisposed to the disease, and the Reder girls particularly exposed to the cause which produced it. Because, moreover, the boxes, as they were discharged from the lighters, were immediately stored in the warehouses on the wharf, and thus in the same way as when shut down and confined under the hatches of a vessel, placed in a situation in which it was impossible for them to give off the fatal poison adhering to them.

"Here then we have detected the disease in its origin. There can be no confusion of dates and names and places; no cavilling about the beginning of the disease, and the first cases of it, a species of subtlety, which has been so often resorted to in previous years, and which to those who did not know the motives by which they who used it were actuated, has for a time passed off as a plausible objection to the doctrine of the importation, specific character, and contagious nature of yellow fever." pp. 27, 28.

Such is the whole train of evidence on which Dr Townsend supports the assertion with which he commences his work,—that "the poison, contagion, or infection of yellow-fever, was introduced into the city of New-York from the port of Havanna, in the summer of 1822." (p. 17.) Having accomplished this part of his task, he proceeds to trace the progress of the epidemic from house to house, and from one street to another. We cannot pretend to conduct our readers, as we have ourselves followed the author, through all the anfractuosities of this devious and somewhat circuitous labyrinth of inquiry; and indeed this is less necessary, as it is by no means essentially connected with the leading position of the author,—that the disease was, in the first instance, transferred from the Havanna vessels to the inhabitants of the city. A Table containing the number of cases and of deaths as they occurred in different streets, which is found at page 39, is succeeded by the following conclusion.

"Thus, then, we see that the disease being once fairly introduced at the foot of Rector-street, it spread from thence at a slow and measured pace over all the lower parts of the city. At first, every case reported could be traced to the immediate neighbourhood of Reder's and Rose's. Then, after it had enlarged a little its boundaries, the cases were traced to the vicinity of those houses. After some days had elapsed, the air of Lumber-street also became embued with the virus of the disease; for an alarming number of persons, the greater part of whom were found to have had communication with the neighbourhood first infected, were all taken sick in this street nearly or about the same time. Then the streets north, south and east of Rector-street immediately adjoining, and parallel to it, began to be included within the limits of infection more or less, according as circumstances favoured its introduction and propagation." p. 40.

The disease subsequently appeared in Cheapside-street, in consequence, as Dr Townsend represents, of a convalescent being conveyed to it from Lumber-street; and it continued to spread through Cheapside and Lombardy-street adjoining, till the frost set in. It is regarded as a strong proof of the infectious nature of the disease, that these streets are comparatively clean, well built, and commodious, and occupied by inhabitants whom he considers as attentive to habits of comfort and ventilation.

"From my own personal examination of Cheapside and Lombardy streets, and especially of the five adjacent buildings in Cheapside-street, where the cases of the Wards, Bailies, and Browns occurred, I can positively affirm, and with the concurrence of every physician who has been to those houses, that there is in no part of the city, five buildings of the same dimensions more cleanly and commodious in their apartments, nor furnished with more spacious and beautiful yards; nor any streets, especially Cheapside, more free of filth or refuse of every kind whatever.

" If the yellow fever of Cheapside and Lombardy streets was generated there, the domestic causes must have been totally at variance with those which existed at Rector-street. There was neither decayed wharves, nor putrid bilge water, nor offensive timbers of shipping, not so much as a single grave-yard within the compass of a whole mile, from which might ooze out the fœtid distillations of human putrefaction! What then were these peculiar domestic causes, which seemed so intense within the narrow limits of these two small streets?" p. 53.

On the facts and reasonings thus adduced by Dr Townsend, it is at once unnecessary and improper to offer a positive opinion. We have in general shunned the alternative of forming such decision as would either preclude further investigation, or prevent us from modifying our opinions on the strength of proper evidence; and we have ever been desirous that authors should be allowed, on such points, to utter their sentiments freely, and in their own terms, and that no improper use should be made either of the facts which they have observed, or of the inferences which they have derived from them. We shall not, on the present occasion, deviate from our established principle, nor regard ourselves in the light of the special pleader, who, with an *ex parte* view of the evidence, is resolved, or is bound, to defend one side only of the question. This resolution, however, does not prevent us from stating our opinion, that the evidence which Dr Townsend has adduced is utterly inadequate to establish his conclusions; and that, be the yellow fever of 1822 contagious or not, either Dr Townsend has not taken the right method of establishing his point, or the facts which he has adduced are of that kind, that they might be explained either on the hypothesis of contagious or non-contagious origin.

We have already noticed the unphilosophical manner in which the author commences his inquiry, by asserting broadly, that the contagion of yellow fever was imported from the Havanna into New-York; and we may now remark, that on this assumption hinges the whole train of his future investigation, and the entire weight of the inferences at which he arrives. It is obvious, indeed, that reasoning, or a chain of argument, is in this case totally gratuitous, and is a mere elegant appendage, to give the whole subject a scientific air. The author's object was accomplished the moment he asserted that a contagious disease, or a disease simply, was introduced from a foreign port to the wharves of New-York; and the whole subsequent train of investigation was completely unnecessary. With such desultory, loose, and inconclusive reasoning, however, we can hardly believe that any one bearing the name of physician would be satisfied; and none will deny, that the author who comes forward with such slender claims to public attention, cannot be expected to make an impression either deep or lasting. The justice of these observations will be obvious to all who reflect on the nature of the inquiry.

We observe, in the *first* place, that our knowledge of the natural history of yellow fever embraces several points, all of which are not determined with the same degree of certainty. Its phenomena have been observed and described, with much care and minuteness, by a variety of authors, from Towne, Warren, and Hillary, down to Hunter, Bancroft, and Jackson; and though considerable difference of opinion actually exists on its proper character, it may on the whole be said, that we are well acquainted with the phenomena with which it is attended. The descriptions and distinctions of Dr Jackson, especially, must be regarded as approaching more nearly to a just delineation of its symptoms than any given by previous authors; and if due allowance be made for those varieties which must occur in the process of every febrile disease, and for those shades of difference which depend on local situation, physical peculiarities, individual peculiarities, varieties of treatment, and influence of exterior agents in general, we think it is hardly possible to mistake or entertain doubts of the nature of the disease. The knowledge of symptoms and phenomena depends on correct observation entirely; and though this observation may be occasionally misled or deceived, it is always possible to correct errors, and to obtain a just view of facts, wherever these are the immediate objects of sensible observation.

It is very different, however, with the question of the origin of a disease, and the mode in which it is formed in the persons of human beings; for so many circumstances which elude the observation of our senses are concerned in this result, that it constitutes, and ever has constituted, one of the greatest difficulties in the art of the physician. The modes in which yellow fever epidemics appear, are so various, so opposite, and are narrated in terms so contradictory, that its origin, above that of all febrile disorders, may be said to be involved in obscurity. It is well known that there are certain places of the globe, particularly the West Indian Islands, and the coast of Mexico, Darien, Guiana, and Surinam; in which it is ever ready to attack the persons of Europeans; it is equally well known, that it occasionally appears epidemically, attacking indiscriminately both Europeans and natives of these places. But there are other situations from which it may be long absent, and which it actually ceases to visit for a series of years, until, from some unseen or unknown cause, it appears all at once, resists every effort made to check its progress, or subdue its violence, and ceases only when it has destroyed a considerable proportion of the population. If to these leading facts we add the collateral circumstances of season, weather, and topographical characters, under which yellow fever makes its appearance in a town or city, many of which are so various or opposite, that they have been employed by different parties to explain the appearance of the epidemic, we have the whole of the facts known regarding its formation, and the manner in which it attacks the persons of men. When with these limitations before us, we reflect on the first appearance of yellow fever in any place, and inquire for a moment into the circumstances on which its appearance may depend, we discover that the doctrine of its contagious origin is at present in the state of a mere hypothesis, which is favoured by some arguments, but contradicted by others;—and which must be eventually refuted or confirmed by more careful collection of unequivocal facts, and by more correct analysis of the inferences which they warrant. It is not understood or asserted by this, that the doctrine of its local or domestic origin is not also in the state of an hypothesis, although this opinion has the advantage of being that, in which the facts are less violently bent to the hypothesis than in the other. All that we contend for is, that the origin of yellow fever, and every such disease, is at present a mere matter of opinion, and not of fact, as several of the partizans of each side erroneously believe; and, that instead of being assumed or taken for granted, as it too frequently has been, it is still to be ranked among those parts of medical literature, which are quite problematical, and which, in the language of strict reason, must be termed *hypothesis.* We know not how far this conclusion will please either party; but we are satisfied of one thing, that it is utterly impossible to peruse the various writings of physicians on this subject, and resist its legitimacy. Medical writers, it may be observed, are seldom careful in distinguishing facts from opinions; and in the investigation of the etiology of diseases, the usual fallacies which attend all inquiries of this nature, have operated with great force and greater effect in preventing physicians from perceiving the uncertain points in the train of investigation, and have often led them to convert into an established doctrine, or an incontrovertible principle, what was at the best, an ultimate and remote conclusion from many facts and arguments.

Secondly;—when the question before us is viewed in this light, we perceive the propriety of examining it by the ordinary tests to which such questions ought to be submitted. To prove that yellow fever, or indeed any disease appearing like yellow fever, is contagious, and communicated by contagion, three things at least are indispensably requisite. 1*st*, that it prevailed, or was prevailing epidemically at the place from which it is reported to have been conveyed; 2*d*, that it was actually conveyed either in the persons of human beings, or in some analogous mode, in which its energy was not likely to be impaired or destroyed; 3*d*, that the same disease was communicated in a certain and unequivocal manner to the persons of those in whom it subsequently appeared at the place, into which it was said to have been imported. On each of these topics, we shall take the liberty of offering a few very short and hasty remarks.

1. That the prevalence of a disease reputed contagious at the place from which it is believed to be conveyed, is an indispensable condition, must be obvious at first sight. Unless we can prove this as a certain and unequivocal fact, there is an end to all reasoning on the probability of its conveyance to any other place. Obvious and incontestable examples of the disease must be seen in the persons of the inhabitants, ere we can admit that it was conveyed from that place. Nor is it sufficient that the disease be not epidemic, but endemial merely in the situation from which it is said to be conveyed; for the very circumstance of its endemic character is almost universally conclusive against its contagious nature. A contagious disease owes its birth to a peculiar morbid poison which requires for its formation many concurrent circumstances, and consequently its operation is only temporary. It is not peculiar to any country, but may, under favourable circumstances, be common to many. It is different with an endemial disorder, the formation of which depends on certain local and physical peculiarities of the regions in which it prevails, and which it is impossible to suppose to exist, where these physical qualities cease to operate. In the present instance, the author informs us, that yellow fever was prevailing at the Havanna, from which, with other West India ports, it is almost never entirely absent. But though this be admitted,—though, in other words, it cannot be denied that sporadic cases of yellow fever may be always found at some of the West India ports, in which especially European strangers are arriving, we doubt whether this be sufficient to prove, that the disease, or the morbid agent on which its

generation depends, can be thus readily conveyed from the places in which it is endemial, to others in which the causes of its endemiality do not exist, or do not operate. It is well known that vessels are constantly sailing to Europe from the Antilles, the Caribbean Islands, and, in short, from all those ports in which yellow fever prevails, and that they are conveying not only goods which must have been within the sphere of its action, and which must, in many instances, have been abundantly charged with the contagion on which yellow fever is believed to depend, but even persons who must have been exposed to the disease; yet we do not find that yellow fever is propagated in these ports in consequence of the arrival of vessels with West India productions. There are certainly many places in South America, in Africa, in Asia, and even in Europe, to which West India vessels are occasionally or frequently conveying cargoes, yet we have not heard of yellow fever being imported into such places. What should cause New-York, or Philadelphia, or any other city of the United States, to differ in susceptibility from other places within the same parallels of latitude, we are utterly unable to comprehend; or why does it happen, it may be asked, that during one season yellow fever appears in the sea-port towns of the United States, and is not heard of for many previous or subsequent seasons? If the yellow fever of the Caribbean Islands be contagious, it ought to be much more uniformly contagious than it has hitherto been observed to be; and we should scarcely expect a ship to quit one of these islands during the summer months, without conveying this disorder all round the globe. In the instance now before us, we should have expected our author, in order to establish his position, to prove that yellow fever was conveyed from Havanna, not only to New York, but to at least half a score of places, to which West India vessels are habitually trading; yet we are not aware that this result has been observed to follow; for with regard to the instances of Pensacola, New Orleans, and St Augustine, on which our author lays much stress, it is obvious that their situation, climate, and local peculiarities, furnish against the contagious nature of the disorder the same objections which arise from those of New-York.

2. We have stated it to be requisite to show, that the disease was actually conveyed in the persons of human beings, for two reasons; 1*st*, It is exceedingly doubtful, if not positively untrue, that the disease can be conveyed either in commercial productions, or in mere articles of clothing; for the perspiration, or other discharges, appear to possess no specific qualities on which the disease could be reasonably supposed to depend; 2*d*, It is quite impossible, we conceive, that the exhalations of a

body dead of yellow fever could communicate the disease; for, if it be communicable in the way of contagion, it must be during life, while the morbid process is going on, and not after death, when chemical changes begin to take place, and when their products must be essentially different from the result of disease. In the instance before us, we have the testimony of the author, that two men had been ill and died during the voyage; but we are not informed either of the exact symptoms which these individuals presented, or of the time when they took ill, or of any other circumstance which might be requisite to satisfy an accurate inquirer. We do not deny that these persons had yellow fever, but our author gives no proof whatever that yellow fever was conveyed by means of their persons to the banks of the Hudson. The vessel, as well as others, appears to have arrived in a healthy state.

3. Though it be admitted that yellow fever could be conveyed in the persons of human beings from Havanna or any other of the Caribbean ports, what proof is there, that it is or was communicated to the persons of the inhabitants of New-York? That several of these inhabitants became the subjects of yellow fever, is undeniable; but that they received this disease from the boxes of sugar, or other articles which formed the cargo of the Havanna vessels, is an inference which requires, for its admission, much more positive evidence than the work before us supplies. Did these boxes uniformly communicate yellow fever to all who came near them? And did the same result follow all communication with the cargoes of all these Havanna vessels? We rather think that the author has some difficulty in establishing either of these events; and they in some measure contradict themselves, since it is impossible to suppose that the disease was communicated from such a number of stocks, and, in point of fact, does not appear to have taken place. These conclusions will be corroborated by the subsequent history of the disease, as given by our author. (See p. 355.)

We submit these desultory observations, not in absolute disbelief of the contagious nature of yellow fever, but with a view to prompt our author or any gentleman with the same opportunities, to investigate this point with a greater degree of strictness, and with more accurate regard to the sources of fallacy, than it has yet had the fortune to acquire. We are indeed particularly desirous that the question should be brought to something like an *experimentum crucis*, in which there may be no alternative, but admitting or denying absolutely the contagious nature of the disease; or, if the subject do not admit of this mode of determination, that it be placed in such light as may explain the phenomena of the appearance and propagation of yellow fever more accurately than has yet been done. While personal observers of the utmost veracity and of undeniable judgment, defend, with equal zeal and in many instances with equal success, the opposite sides of contagious, and domestic or local origin of yellow fever, it is either impossible or exceedingly difficult to know which side is in the right, or whether both are not in the wrong. After perusing the able writings of Sir Gilbert Blane, Sir James Fellowes and Sir James MacGrigor on the one side, and the not less able works of Rush, Bancroft, Jackson and O'Halloran on the other, it becomes next to impossible to say how far each side has taken the right view of the subject, or whether one of the parties has not been deceived in the examination of the facts, and the formation of inferences.

To return to our author:—we could have wished that he had confined himself more scrupulously and closely to what fell under his own immediate observation in the New York Epidemic, or at least that he had not gone beyond the testimony or observation of his own friends. It is only by the direct personal observation of a limited number of facts, that any solid conclusions can be formed regarding the causes on which they depend, and the agents concerned in their formation. On this account, we cannot see any good reason for referring to the opinions of authors on the source of the epidemics of Grenada, Barcelona, Malaga, Cadiz, Gibraltar, &c. when it is well known that writers have varied as much about the origin of the disease in these places, as about any that has ever appeared in New-York; and we regard the whole of what Dr Townsend has advanced on other authority, or quoted from other writers, from p. 67 to 78, or what he terms 'the overwhelming testimony of the Spanish physicians,' and the further extract from the Report of Dr Beck, as of no force or moment whatever to prove the contagious character of the New-York fever. The testimony of the Spanish physicians, on which our author lays so much stress, is in truth nothing more but a collection of opinions delivered exactly in the manner in which opinions of this description are, unsupported by facts, or at least by facts which are either doubtful, ambiguous, or contradicted by others equally strong. We could have wished that our author had examined attentively the writings of Drs Jackson and O'Halloran, in which, if he discovered not a contradictory testimony to that of the Spanish physicians, founded on materials certainly of equal stability, he would have rendered a material service to the question, in teaching us in what manner the evidence of these assiduous and courageous observers was defective or incompetent to establish their conclusions. Dr Townsend ought further to be aware, that several of the Spanish physicians, who believed strenuously in the imported origin, and contagious nature of the Andalusian yellow fever, have now changed their opinion, and in so doing, have afforded, to the champions of domestic origin, all the confirmation which such conversion could communicate to their hypothesis. We will not however insist, on this mode of deciding a scientific controversy; and we lay it down as a fixed principle, which must be obvious to all, that a question, regarding the origin of a disease, and its propagation by contagion, can never be decided by appeal to authorities, or by extracts from works which, however estimable in other respects, contain at best but opinions and not decisive observations. We may indeed in this manner learn, that 10 physicians give an affirmative opinion, while 10 or even 12 give a negative one; but it is obvious that the true nature of the question is not altered by this equality or majority of opinion, and that it is utterly impossible in this manner to come one single step nearer to the truth. It is indeed perfectly astonishing, that the nature of the evidence requisite to establish a medical inference of this kind, should be so grossly mistaken, and that physicians should bring forward, in confirmation of their opinions, not facts, but merely the opinions of other physicians of the same mode of thinking. But hear the summary manner in which our author winds up his discussion on this question, and terminates his inquiry by the following sweeping exclamation:

> "What need have we to collect individual facts to prove the contagiousness of Yellow Fever, when the general history of its progress, from its introduction into the city, to its extermination by frost, constitutes of itself one entire mass of overwhelming evidence in confirmation of this truth?" p. 84.

Our author goes on nearly in the same style to refute some of the objections of Dr Bancroft, partly by observations of his own, partly by quotations from Dr Hosack's work on Medical Police, and especially a letter addressed to this physician, by Dr Joseph Bayley of New-York, and terminates in a peroration, of which the following extracts may be taken, as fair specimens.

> "We have now concluded all that we had to offer of a general nature in the way of objections and arguments, against the domestic origin of yellow fever; as it has appeared in previous years in the northern latitude of the United States, and more especially as it has appeared this year in the city of New-York. As we have said before, the general history and outlines which we have furnished of the progress and march of the disorder, first in the lower part of the

city, and afterwards its introduction into, and propagation through, Cheapside and Lombardy Streets, the details of which agree in every respect with what has always been observed to hold true of yellow fever in previous years, and particularly of those diseases which are acknowledged by all, to be contagious, ought to convince any person, whose mind is not clouded with prejudice, that yellow fever is truly and justly entitled to be ranked as a contagious or communicable disease, propagated from one individual to another, by the power of its own specific poison, and characterized by symptoms which distinguish it from all other affections. Every other mode by which we attempt to explain the origin and progress of yellow fever, however ingenious and plausible it may at first sight appear, leads us, as we shall see when we enter into a close investigation and comparison of facts, with the principles or doctrines of the theory which pretends to account for them, into so many absurd and preposterous conclusions, that we find ourselves under the necessity of abandoning it as totally inadequate to explain the phenomena under consideration.

"We find ourselves at last compelled to acknowledge, that the contagious property of yellow fever, is that alone which will rationally and satisfactorily account for the peculiar circumstances which attend its introduction, and propagation over different parts of the town. In order, however, to indulge this species of mental idiosyncrasy, which affects not to understand, or to tolerate any theory on this subject, that is not substantiated by an over-minute detail, however trivial, of every circumstance of which the senses are enabled to take cognizance, and which seems to be incapable of making those broad deductions and bold generalization which, as Lord Bacon says, distinguish the minds of those who are deemed to hold an empire over society, and "to handle the affairs of men," we will furnish a table and catalogue of particulars, which, we venture to affirm, are sufficient to reconcile even the scrupulous nicety and delicate perceptions of this microscopic class of philosophers. We call upon them with their elaborate machinery of dates, and names, and circumstances, to produce more positive, unambiguous, and detailed testimony, in favour of the contagious nature of any disease, than that which we shall soon offer in support of the *contagiousness of yellow fever.*" pp. 98–100.

This vaunting challenge is succeeded by a comparative table of the cases, favourable or fatal, as they occurred in streets in which the disease prevailed. The whole space of the city in which yellow fever appeared, is divided by our author, according to the views which he entertains, into the lower, or originally infected district, and the upper, or the secondarily infected district. Within the first space is comprehended that part of New-York which is contained inclusively between Rector-Street and its neighbourhood, Broadway, and the contiguous streets of Carlisle, Beaver-lane, Albany, &c. and Courtland-Street, &c.

east of Broadway, with some of those more ancient parts which were built by the Dutch settlers of New Amsterdam, as it was then named. The whole of this part of the city consists of narrow, ill-built and incommodious houses, several of them wood, and the wharfs which adjoin are entirely log-built. The upper, or secondarily infected district, is very different. It consists of Lombardy Street, Cheapside, and Bancker streets, which are comparatively more spacious, and consist of houses which are, on the whole, larger and better built. The only general fact which it is important to notice is, that the whole number of cases amounted to 422, of which 295 occurred in 86 houses, all of which were either opposite or contiguous to each other, and contained from 2 to 8 cases, or at an average $3\frac{4}{11}$ nearly, while the number of individual cases in separate or individual houses was 86. This our author regards as the strongest and most irresistible argument of the contagious propagation that could be adduced, and rings 150 different changes upon it, in the most irregular and confused manner, in order that the ears of his readers may become perfectly familiar to the great purpose of all his reasonings,—contagious propagation. Now, it does not appear that this fact carries along with it such an alarming degree of force as to strike down all the opposers of his doctrine, to the confusion and dismay of the advocates of non-contagion; for it was perfectly well known long before Dr Townsend undertook the inquiry, that yellow fever, when prevailing epidemically, always appeared most abundantly in the same neighbourhood, in the same street, and generally in the same house; and this circumstance, instead of being regarded as an evidence of contagious propagation, was, in general, explained with great facility on the hypothesis of the local and physical peculiarities of the place, and, in many instances, was held as proof of the disease having arisen from domestic causes. Our author seems to overlook completely the circumstance, that yellow fever, neither in the epidemic which he describes, nor in that of other places, affected uniformly those persons who were most exposed to it, and that both the attendants of the sick, and others who were frequently about them, were by no means regularly or abundantly affected, and even, in most instances, escaped. It does not appear that any of the medical attendants in the epidemic now alluded to, suffered even an attack of yellow fever, and the account of Dr Townsend himself bears proofs of some very strong facts against it.

We allude to the fact which our author himself, with a most creditable degree of candour admits, that both furniture, clothing, &c. and even persons with sickness on them were conveyed from the infected district to the suburbs, or the village of Greenwich; and yet no cases of yellow fever were transmitted in consequence of this conveyance. (p. 63.) We regard it as quite immaterial to distinguish, as our author has attempted to do, between the circumstances of these persons, and those of Ward, on which he insists so strongly; for although it be granted to our author, that this person entered a healthy part of the city with a double portion of morbid contagion, both that of his person, and that of his dirty clothing,—it is impossible to comprehend why individuals labouring under the disease should be less capable of communicating it than one who was at the same time accompanied with clothing and bedding, which, however contaminated, had been removed from the source of contamination,—the person of the individual. If these persons were removed while the disease was still on them, which Dr Townsend asserts they were, they were the living engines who were incessantly producing and multiplying the invisible agent, on the extent and quantity of which the propagation of the disease was believed to depend; and if this invisible agent were truly contagion, several instances at least ought to have occurred, in which these persons were still capable of communicating the disease. (p. 64.) This part of our author's treatise deserves most serious attention from all who either credit or deny the contagious propagation of yellow fever; since the circumstance of the disease not being communicated in consequence of these migrations, is one of the strongest proofs of its local origin that can be adduced.

Another very strong fact on this side of the subject is, that 104 persons were actually sick of yellow fever in parts of the city, which our author terms uninfected;—of these, 65 did not reside in the district supposed to be infected, but had either been employed in working there, or had only passed through it, and the remaining 39 were scattered over various parts of the city, which, according to the hypothesis of the author, were not infected. Now, it must be admitted, that there are only two modes in which this arrangement of the epidemic can be explained: The 1st, either by supposing both the 65 persons and the 39 to have received the disease directly from the infected persons, or from materials in the infected part of the town, though there is no proof of this; or, 2d, that a much greater part of the town than our author admits, was in circumstances capable of communicating the disease; and, consequently, that yellow fever could be communicated, where no contact between the subjects and the articles imported from the West Indies, could be traced. That the latter hypothesis is much more probable, must be inferred from another fact, for which we are indebted to the candour of our author. This is, that of the 104 persons thus diseased and not residing in the infected parts of the city, not one communicated yellow fever either to physician, nurse, or other attendants, or even to the inhabitants living in the adjoining houses. When our author admits it to be impossible to explain this peculiarity, we presume he means on the hypothesis of contagion, and contagious propagation only; for we have seen similar peculiarities explained in a plausible manner, according to the doctrine which ascribes yellow fever to endemial causes, though we are unable to say how far this explanation may be true, or consistent with all the phenomena of this disease.

The chapter is concluded with 22 pages of "examples of the contagious nature of the disease," derived either from our author's personal observation, or contained in letters from several of his correspondents, professional or otherwise, or Reports from the New-York Board of Health. Several of those cases had been already detailed or mentioned in former parts of the chapter; all of them are of the same character, and liable to the same objections which we have stated generally against our author's mode of reasoning;—we refer to the work for particular information.

We fear that we have completely succeeded in tiring the attention of our readers with this part of our author's work: and we assure them, that it is impossible for them to feel greater joy, than we ourselves experience, on concluding the chapter, and the subject of which it treats. While we utter a congratulation, however, which we trust is reciprocal, we feel that we have great merit in the patience with which we have perused the chapter, and selected from it those parts which appear most worthy of being known. Though we have bestowed on it more attention than either the argument or the information contained in it may reasonably warrant, yet this attention was due to the subject of yellow fever, and more especially when treated by a countryman of Dr Rush. As the author had personal opportunities which fall to the lot of few, of investigating the disease of which he treats, we naturally regarded his work as founded on the most authentic documents, and as conveying the most correct and enlightened views possible, and expected to find in it those clear and decided proofs of the contagious nature of yellow fever, for which we have hitherto looked in vain. How far these expectations have been realized, we think our readers are now quite as capable of judging as we are; and we think that, unless they surrender their reason to mere assertion, and give to assumption and declamation that confidence which the evidence of indisputable facts alone ought to claim, they will agree with us in concluding, that in whatever mode yellow fever appeared at New-York in 1822, or by whatever channel it was introduced, the work before us furnishes little or no proof that it came from Havanna any more than from the inhospitable air of Cape-Horn, or the snowy summits of Chimborazo, and none whatever that it was contagious in its propagation, or in the course which it afterwards observed. We have, however, dwelt long enough on this subject, and we hasten to consider the merits of our author's further disquisitions.

The "Analysis of the Symptoms," in the 2d chapter, is a description of the state of the functions and other properties of the living body under the influence of yellow fever, during the periods of invasion, first, second, and third stages. It is minute and sufficiently accurate, though the arrangement might have been more clear and natural than it is.

In settling the pathognomonic symptoms (chapter 3d), our author seems to experience as much difficulty as any of those writers who have preceded him. He admits that, as all fevers commence in a similar manner, the practitioner can rarely form more than a probable opinion at the first; and yellow fever requires to have existed for some time before its character is distinctly marked. On the second or third day, however, at the farthest, he thinks a conclusion may be positively formed. This may be true with regard to yellow fever as it occurs in New-York; but in other situations, if the mind of the practitioner be so long undecided, it will matter little whether he decide at all.

Dr Townsend afterwards proceeds to enumerate pathognomonic symptoms in the state of the tongue, the eye, the pulse, the stomach, pain in the orbits, hind-head, back, loins and limbs, and the languor or faintness of the general strength, and the sighing with which respiration is performed. The morbid conditions thus enumerated are not examined in a very methodical order; but they correspond, in general, with sufficient accuracy, to the accounts given by the best writers and practical observers. Our knowledge of this disease would lead us to place most reliance on the state of the epigastric region, and of the stomach, which is uniformly, immediately after invasion, tender, sore, and oppressed with squeamishness, and indescribable anguish;—on the state of the pulse, and the successive changes which it undergoes, along with the heat of the surface; and, lastly, on the nature of the epidemic, and of the season. If proper attention be given to these circumstances, it will rarely be difficult to perceive the true character of the disorder.

In the 4th chapter, Dr Townsend considers the characters by which yellow fever may be distinguished from many other diseases with which we should think it would not be easy to confound it. It may, under certain circumstances, approach very closely to remittent fever, of which many physicians regard it as a variety; it might be mistaken for typhous fever, to the head of which some nosological writers of no ordinary eminence have referred it; the acute pain of the head with which it commences may be mistaken for phrenzy; and the soreness of the pit of the stomach, and frequent vomiting which it almost invariably causes, may lead an inexperienced observer to think on inflammation of the stomach. But we can hardly regard it as within the compass of probability, to confound a disease like yellow fever either with continued fever, or with ague, plague, enteritis, hepatitis, catarrh, asthma, or even with intoxication. From each of these diseases, it may surely be easily distinguished, by attending to the mode in which the patient is attacked, the kind of symptoms, and especially the course which they follow. On this subject, however, we deliver our opinion with that deference to our author, to which his greater experience, and more intimate acquaintance with the modifications of yellow fever justly entitle him. He has very probably had occasion to witness the facility with which it may be confounded with all the diseases from which he has given characters to distinguish it; and we will not be chargeable with the impropriety of doubting his judgment and discretionary power.

It is, however, agreed among physicians of experience, that the difficulty of distinguishing yellow fever from remittent fever as it occurs in tropical countries, and during the warm seasons of countries contiguous to the tropics, is by far the greatest and most perplexing; and the desire of seeing and showing how our author acquits himself of this delicate task, compels us to bestow a little attention on it.

" When the stage of collapse," says Dr Townsend, " arrives in yellow fever, the hasty observer is apt to imagine, from the regular and nearly natural pulse, and the cool temperature, softness, and moisture on the skin, that a perfect apyrexia, or absence of fever, and an entire solution of the disease, have taken place, until the unnatural slimy fur and margin on the tongue, the frequent sighing for breath, and, above all, the burning irritation at the præcordia, particularly on pressure, show the physician that all is not going on right, but that he has more danger, perhaps, to apprehend from this contradictory assemblage of symptoms than if they existed separately. He will be still more deceived if, under the erroneous impression that yellow fever and remittents are descended from the same parentage, he waits to see a return of the exacerbations and remissions." pp. 179, 180.

Now, it is agreed by all physicians who are practically acquainted with the phenomena of the tropical remittents, that the burning sensation of the epigastric region, with coolness of the surface, is almost uniformly observed in these fevers; and consequently, that it cannot be regarded as a sure criterion to distinguish yellow fever from these diseases. It is further agreed, especially by those who have observed the Mediterranean fever, that apyrectic intermissions are not uncommon, and have not unfrequently led both physician and patient to believe that the disease was cured, or had subsided.

The subsequent observations which our author delivers on yellowness and bilious symptoms, are quite out of place; and Dr Townsend appears to have lost sight of his diagnostic inquiry in something like an explanation of the cause of yellowness, and of the bilious matters which are discharged from the stomach, and sometimes from the intestines, in yellow fever. On this head we have merely to remark, that a short and convenient distinction between yellow fever and remittent, might have been found in the nature of the matters discharged. In yellow fever, though it is first mucous and watery, and then bilious, it soon becomes brown, dark-coloured, or like coffee-grounds—constituting the black vomit (*vomito prieto*) of the Spanish-Americans; in remittent fever, the matters vomited are generally distinctly bilious, or bile mixed with the food and drink; and if black vomiting takes place, it is at the conclusion of the disease only, when it is near a fatal termination. Our author concludes his observations on this subject with a quotation from Dr Warren; whose memory he congratulates, that he lived at a time when the identity of yellow fever and remittent fever were never thought of.—It is truly unfortunate that Dr Townsend is so fond of mistaking authority for evidence, and opinion for fact; and we think he would have rendered his distinctions much more trust-worthy and valuable, if he had shown satisfactorily that they were derived from long, varied, and careful personal observation.

The opinion of the communicable nature of yellow fever, appears to operate most powerfully in preventing the examination of the state of the internal organs after death. Our author does not appear to have made any dissections personally, and has given the particulars of four cases only, which were examined by other physicians. Of these we select one, which we regard as the best and most instructive, in which death had taken place on the fifth day of fever, and the body was examined two hours after.

"Extravasations of a livid colour were noticed in the lips, ears, blistered surfaces, scrotum, &c. The cellular membrane, the stomach, the liver and its substance, the mesentery, dura mater, apex of the heart, pleura, peritoneum, and internal membranes generally, were tinged with yellow. The blood in the heart was fluid; *the lungs were very brown or black*; the liver was unusually tender, and seemed small; the omentum and dura mater appeared a little congested; the glands of the mesentery were somewhat enlarged; the spleen was natural; the bladder was nearly *empty*, the patient having *made no water for thirty-six hours before his death*. The gall-bladder was neither empty nor full. The patient had had *no black vomiting*, but the stomach was found to contain about *a pint of the matter of black vomit*. The villous coat of the stomach was lined with a coat of black matter or bloody extravasation, and its texture was so very tender that it was easily removed. The brain and spinal medulla looked quite natural; they were without marks of inflammation, but seemed slightly congested externally." p. 190.

The pathological principles of Dr Townsend, though not peculiar, deserve however to be remarked. They will be understood from the following passages.

"We may be said to have this year seen yellow fever in its most simple and unadulterated form, which has thus placed it in our power to determine the real position which it occupies in the system, and the ruling tissues that sustain the brunt of the disease. If the experience of the present year ought to have any weight, it has been, in our opinion, clearly established, that yellow fever is fundamentally and *primarily* a disorder of the nervous system. From the regular period of time which, like other contagious diseases, it usually requires for the poison to operate after exposure, it is nevertheless probable that the fluids may serve as the *medium* of its introduction into the system. It seems probable that, like most contagious diseases, it is first inhaled through the nose and mouth into the lungs. Some think it innocuous to the system in its natural state, but that it becomes deleterious on being decomposed by the secreting vessels at the sentient extremities of the arteries, where, in its now altered state, it begins to show its effects first on the nervous system. I cannot think this opinion true, for the affection of the eye, forehead, and tongue, in the first stage, and the epistaxis afterwards, obviously lead to the inference, that these symptoms are rather to be attributed to direct absorption of the poison on its passage through the mouth and nose, than to its influence upon those parts after being changed by the action of the fluids. The affection of the stomach, as well as the hæmorrhagic tendency in the extreme vessels, arrive at a much later period, and therefore, perhaps, show that the poison produces these effects indirectly, by passing first into the circulation; thus proving also, if my first assumption be correct, that the *materies morbi* is not altered in its passage through the blood. Apparently, the pathology of the disease changes; for the nerves first bear the *onus* of the disorder, and then, towards the close, the vessels and their fluids, as manifested by the general tendency to hæmorrhage, the dissolved condition of the blood, and *entire* absence of pain in those parts which were affected in the beginning. We have seen that, in a great number of fatal cases, there has not been the slightest hæmorrhage from the stomach, in many others none or very little from the external passages, and in but very few instances an inordinate quantity, or vitiated condition, of the bile and other secretions; that the excretions, as the sweat, saliva, breath, urinary and alvine evacuations, unlike typhus fever, to which it has been compared, have all been, for the most part, untainted with any peculiar or offensive odour, and most usually unchanged in their character; in fine, that the chylopoietic, and assistant chylopoietic viscera, have often performed their offices with a precision and regularity which, to those who have heretofore thought the fluids or their vessels the *nidus* of the disorder, has appeared almost miraculous." pp. 198–200.

" That a morbid irritability of the stomach was always present, and that it invariably increased as the disease advanced, without however at any time excessive retching or vomiting, was obvious to all. But does not the highly organized condition of the stomach, and the vast supply of nervous energy which it receives, together with the frequent absence of hæmorrhage, prove that this irritability was more a *nervous* than an *inflammatory* affection? Does it not prove, and do not also the regularity of the abdominal functions generally show, that yellow fever has no more to do with gastritis than with remittent fever or with hepatitis or enteritis? If the irritability of the stomach was a pure inflammatory affection, and a symptom of gastritis, surely this ought to be greatest on the first day of the disease, for it is then only that the arterial excitement exists, and not come on afterwards, when the vascular system has lost its force, and the relaxed capillaries are pouring out drops of blood and sweat." p. 201.

" The frequent occurrence in this disease of a particular species of mental derangement to which we have alluded, as also the pain in the back, loins and legs, the sighing and lassitude and hiccup, and the slow depressed pulse more especially seen in convalescence, together with the peculiar location of the pain about the back, loins, forehead, eyebrows and eyes, the occasional constipation of the bowels, and frequent suppression of urine, as also the occasional dilatation of the pupil, and the constant affection, and sometimes partial paralysis of the tongue, and, above all, the gastric irritation, go very far in support of the opinion, by no means novel or original, that yellow fever is, in fact, a disease of the nerves. Does not this opinion also account for the exemption of the organs of voluntary motion and the vascular tissue of the lungs, liver, spleen, and kidneys, from any particular irritation or inflammatory action, these organs being comparatively destitute of nerves when contrasted with the coats of the stomach?

"Does not also the black or dark colour of the substance of the lungs, frequently found after death, show that their nerves have been partially paralyzed, and that the decarbonization of the blood was imperfectly performed? The same as is seen also in autopsic examinations of those who die from the bite of certain venomous animals, and to the poison of which that of yellow fever may be analogous?

"And, lastly, does not this explain also the remarkable symptom of *sighing* in this disease, and which, in all probability, is an involuntary effort in the respiratory organs, to remedy the disordered condition of their functions, and, like the slow measured pulse of convalescence, the dilated pupils, the suppression of urine, and the passive hæmorrhages attributable to the sedative operation of the poison?" pp. 203–5.

Now, if by *affection of the nervous system,* or *nervous disorder*, our author means, general derangement of all the properties of the organic tissues, and disorder of every function which depends on the sound state of these tissues, assuredly he is not far from the truth, or, at least, he announces a fact, which to a certain extent takes place in every case of yellow fever. But that yellow fever consists in a disease of the brain, of the spinal chord, of the nerves of the lungs, or of the nerves of the stomach and intestinal canal, is a conclusion which, we think, few sound pathologists would draw, and with which few sound practitioners would be satisfied. That the properties of the stomach or gastric tissues are much altered, is undeniable; that the brain differs much from its sound condition, is equally undeniable; but it appears more consonant with correct views of morbid action to think, that these morbid states of the brain and stomach consist in the change which takes place in their capillary circulation, and, perhaps, in their organic properties in consequence, than in any unsound state of the nerves of the stomach, or of any other part. The state of the lungs is evidently dependent on the impaired power of the muscles of respiration, and on the consequent difficulty which the blood encounters in passing through the small branches of the pulmonary artery and pulmonary veins.

In conducting the treatment of yellow fever, Dr Townsend deviates widely from the rules delivered by physicians of eminence and experience. He is an obstinate antagonist to blood-letting, at least in any quantity, and employs it in an extent too trifling to exercise any effect on the disease. According to his experience of the New-York epidemic, most benefit was derived from "active doses of calomel in the beginning, followed by saline, alkaline, and emollient cathartics, frequently repeated in the course of the disease, with strict attention to the skin, by frictions, stimulating embrocations, warm ablutions, and mild tepid drinks, so as to withdraw the excitement and fluids to the surface and to the bowels; and by keeping these important passages of the system open, give every opportunity for the disease to escape.

"The early application of blisters to the epigastrium, chest, back of the neck and extremities, in order to anticipate by counter-irritation upon the surface, the dangerous affection of the stomach which almost constantly supervened as the disease advanced, was in many instances found advantageous. Too often, however, fomentations, rubefacients, sinapisms, and other temporizing measures, were trusted to as substitutes, while the more efficacious mode of freely blistering was timidly withheld until it was too late to do good; the blood having retired from the surface to the internal parts of the body, and the irritation of the stomach advanced so far, and the tone of the circulation become so much impaired, that no benefit could be derived from this application." pp. 209–10.

Both our author, and some other physicians of New-York, appear to conduct their therapeutic measures rather with regard to individual symptoms, than to general views of the nature of the morbid process of the disease. It is on this principle only that the exhibition of diuretic medicines can be explained. The most powerful diuretic, in such circumstances, will be found to be a bleeding from the system to the amount of 35 or 40 ounces, aided by the effects of the warm bath, and other means, to restore the circulation and secreting function to its natural condition.

Emetics are justly condemned, at least when they exercise an effect merely emetic. Arsenic has been said, our author affirms, to be successful in an epidemic which occurred on board of the Macedonian, during a cruise in the West-Indies; but in consequence of the death of the surgeon, details have not been obtained. Our knowledge of the effects of this medicine leads us to think, that the length of time which it requires for its operation, would render it useless, unless given so frequently that its exhibition must necessarily be dangerous.

The 7th Chapter, on Prevention and Purification, is long, and somewhat elaborate, and, like other parts of the work, is much supported on authorities: among which, the report and opinion of the Spanish physicians, the work of Sir James Fellowes, but especially the Discourse of Dr Hosack on Medical

Police, stand preeminent. As our author's reasonings are diffuse and rather vague, it becomes a matter of difficulty to concentrate his actual principles; and as his knowledge of the nature of the insensible cause of yellow fever is not more precise than that of his predecessors, his measures are undecided and wavering. He evidently regards it as of a gaseous nature; but in applying chemical laws to explain its operation, he is neither clear nor consistent. He admits, that the most effectual method of disinfecting sickly air, next to frost or cold, &c. is admitting that which is pure; but he appears to overlook entirely the noted fact, that the mode in which these two agents operate, is perfectly opposite; as the first must concentrate and condense, while the second diffuses, spreads, and diminishes. It is certain that they finally produce the same result; and it is not improbable, that this might be explained more clearly by a more accurate knowledge of the agent on which yellow fever depends; but so long as this continues so little known as it is, we cannot hope to apply the reasonings of chemical laws with great justice or success. Akin to this strange speculation is the absurd opinion, that frost decomposes the elements of this unknown agent. What proof can our author adduce that frost ever decomposes? We have known it condense, but never decompose; and it is generally regarded as the most powerful agent in arresting or preventing decomposition, both in solid and gaseous bodies. It is truly tiresome to listen to the extraordinary fancies in which our author indulges on this head, and we dwell no longer on them.

The belief which Dr Townsend so firmly entertains in the contagious power of yellow fever, leads him to recommend a strict code of quarantine regulations, and the establishment of lazarettos and hospitals, in the strongest terms. Nothing short of the most vigorous and energetic measures to prevent the communication of suspected persons, articles or vessels, will satisfy him; and he remonstrates loudly against the languid and imperfect manner in which the present quarantine laws are enforced by the American government. It is obvious, that the propriety of all measures of this description must depend on the efficacy which they have in preventing the introduction and propagation of the disease; and if it could be clearly proved that contagion was the source and origin of the evil, we doubt not that the proper authorities will see the propriety of adopting the suggestions of our author. After strengthening this part of his subject by the authority of Dr Hosack, whose character and opinions he benevolently and fondly eulogizes, he concludes by recommending the establishment of a

Board of Health, composed of respectable citizens, with a minority of physicians, whose duty, he suggests, should be to inspect the health concerns of so many districts of the city. We do not pretend to offer any opinion on the merits of this proposal, which appears sufficiently reasonable, though its propriety must be decided by numerous circumstances which we have no means of knowing or estimating.

The 9th chapter, on the subjects of Immunity and Predisposition, is the most interesting in the volume. It may be distinguished into two parts, one treating the question of immunity from yellow fever, after the first attack,—the other regarding that immunity which is supposed to reside in the persons of those born in the countries where yellow fever is endemic. The author commences, with stating the authority of Dr Pym, Arejula, Sir James Fellowes, and Sir Joseph Gilpin, to show that the yellow fever of Andalusia has never been known to attack the same person twice. This is followed by a testimony from Dr Francis, by letter to Dr Hosack, containing an extract from the appendix to Dr Pym's Observations, and an answer of Sir J. Gilpin to the queries of the Army Medical Board, —extracts from Dr Cathrall, and Dr Currie, from the Report of the Army Medical Board on Dr Pym's book; and, lastly, an opinion of Dr Walters of New York. We cannot enter into the merits of the opinions derived from these various sources, all of whom agree in regarding yellow fever as a disease which never makes a second attack in the same individual; but refer our readers for details to the work itself, and to the writings of those who have distinguished themselves in advocating this opinion.

On the second species of immunity, or that which is conceived to depend on birth-place, our author furnishes information by no means undeserving of notice.

> " With regard to this *immunity from birth-place*, it is now almost reduced to a certainty that it does not strictly belong to the inhabitants of any part of the territory of the United States, even to those parts of our continent which are nearest to the tropics. There is, however, a certain degree of unsusceptibility to this disease, which the natives of the United States possess, derivable, no doubt, from this cause, and existing in a more and more perfect state the nearer they reach to the equator. But this does not amount to that positive and absolute immunity to which we have reference, and which is possessed in so eminent and remarkable a degree by the natives of those places where the causes which generate this disease are known to exist. Hence it happens, that although those who have migrated to the United States from the northern and higher latitudes of Europe,

constitute but a minimum of the population of any of our sea-ports, they have, notwithstanding, always been peculiarly obnoxious to yellow fever, even though they may have resided here several years, as is shown by the epidemic of the late season.

" The disease spread over the lower part of the city where the wealthiest portion of the inhabitants reside, almost all of whom are natives, and where there are very few foreigners ; for this description of persons being chiefly Irish and poor, dwell almost exclusively in the suburbs and upper parts of the town. Yet, as we shall soon see, nearly one half the deaths were foreigners. The same has been observed when yellow fever has prevailed in any of our cities. These facts show that the unsusceptibility to yellow fever growing out of birth-place belongs, in a certain degree, also, to the natives of the continent of North America, as well as to those who inhabit its islands in the Carribean sea.

" That some of us do not possess this immmunity in a higher degree seems strange, when we recollect that Pensacola is in 30° 30′ north latitude, New-Orleans and St Augustine in 29° 58′, and Havanna in the island of Cuba, 23° 12′, which is only a little more than *six degrees* farther south than the two last mentioned sea-ports of the United States. But that this peculiar difference between the islands of the West Indies as well as the towns on the continent within the tropics (such as Vera Cruz, Carthagena, &c.), and the sea-ports of the United States, does actually exist, every year, almost, furnishes strong and positive proof; and particularly in what occurred this very season at the little town of Pensacola. Though the greatest number who perished of the yellow fever which recently made such dreadful havoc in this small town, were emigrants from the northern states ; the old residents and natives also, such as the Spaniards and French, and even the Creek Indians themselves, who are the aborigines of the soil, as well as a considerable number of Negroes, fell victims to the disease! The epidemic broke out on the 25th of July, about ten days after the arrival of a vessel from Havanna, that came directly up to within a few feet of the town, and landed a cargo of damaged fruit. There were then one thousand inhabitants. Six hundred fled a few days after into the woods, leaving about three hundred Spaniards, French, Negroes and Indians, and one hundred Americans, Irish and English. Of these there perished up to the 1st of October :

200 whites,	100 American and Irish, 100 French & Spaniards,	Adults and children,
50 Negroes, 30 Indians,	50 Negroes, 30 Creek Indians,	Adults and children.
280	280	

" It is remarkable that from Oct. 1st to Oct. 26th, out of the three hundred of all descriptions then remaining in town, there were one hundred and twenty who had died, nearly all of whom were children of Creole (i. e. native) parents, and who on that account, from being natives of the town, had been considered safe !

" It is conjectured that out of more than four hundred cases of yellow fever which have occurred this season, none have had the disease before. One half, at least, were foreigners from England, Ireland, Germany, or France, or persons from more northern parts of the United States, and who had never had yellow fever. Of the remaining half, no instance has come to my knowledge, of the disease having attacked the same person twice.

" The total number of deaths of yellow fever in the city, recorded on the books of the City Inspector, Dr Cumming, during the summer and autumn of 1822, was one hundred and sixty four.

" They were from the following countries and states :

Foreigners.		*Americans.*	
Ireland, - -	32	New-York, - -	58
England, - -	26	Connecticut, - -	14
Germany, - -	9	Massachusetts, -	7
Scotland, - -	3	New-Jersey, - -	7
France, - -	2	Pennsylvania, -	1
Holland, - -	2	Maryland, - -	1
		Rhode-Island, -	1
Total,	74	Nova Scotia, - -	1
	90		
		Total,	90
Deaths,	164		

" Of the ninety Americans, the following came from latitudes much higher north than that of the city of New-York. Thus, from

Connecticut, - -	14
Massachusetts, - -	7
Rhode-Island, - -	1
Nova-Scotia, - - -	1
	23

" Out of the whole number of deaths, in New-York, during the late epidemic,

5	were between	1 and	10 years of age.
17	—	10 -	20
40	—	20 -	30
40	—	30 -	40
36	—	40 -	50
15	—	50 -	60
8	—	60 -	70
2	—	80 -	90

" Of which six only were coloured persons, of whom three were blacks and three mulattoes. There were fifty-nine females. The number of females was therefore to that of males a little more than one to three.

"Out of the whole number of females, it is worthy of remark, that twenty-three were foreigners, and eleven from the more northern parts of the United States, making the *ratio of susceptibility* between those of northern constitutions and those who are acclimated, hold nearly true in respect to females also, as well as to both sexes, when included together.

"Of the whole number of deaths, *two thirds* it appears were between the ages of twenty and fifty; and of the remainder nearly as many under twenty as over fifty. There were more than three times as many between ten and twenty as between one and ten; and nearly twice as many between fifty and sixty as between sixty and ninety. It is a little remarkable that very nearly the same numbers died between twenty and thirty, between thirty and forty, and between forty and fifty.

"A number of blacks, who had been employed as watchmen, or to take furniture and goods out of the infected parts of the town, were thus particularly exposed to the disease long after the original inhabitants of the district had deserted it. Dr Lining says he never knew an instance of a black taking the yellow fever at Charleston. There were much fewer of the night watch who took the disease than of those who were employed also on the day watch. Dr Rush attributes it to habitual exposure to the cool night air. There were also five or six Germans took sick, who continued constantly to work in a sugar-house in Liberty-street, near Nassau-street, long after that neighbourhood had become infected—they lived in the upper parts of the town, and several of them died." pp. 248–253.

These are the most important results relating to that sort of immunity, which is believed to depend on the unknown influence which the birth-place or the residence of the individual communicates. It is evident, from the testimony of Dr Townsend, that it constitutes an immunity within certain limits only, that it may be much impaired or entirely destroyed in the natives of tropical regions, by long residence in temperate or northern climates, and that though on ordinary occasions, persons of colour escape, considerable sickliness and mortality may take place among them during the prevalence of an epidemic.

We cannot enter into the subject of the 10th and 11th chapters, on the weather and collateral diseases, since they would lead us farther into detail than we pretend to go; and we could not venture, either with justice to our author, or advantage to our readers, to abridge them. The conclusions which are contained in the 12th chapter, are much biassed by the hypothetical notions of the author; but, may nevertheless be recommended to the attentive perusal of the reader, as affording a short but complete summary of the subjects discussed in the previous part of the work. The appendix contains a multitude of documents from official persons, public bodies, or professional individuals,—all proving most strenuously the same opinions which our author entertains. After the minute account which we have already given of these opinions, we look on ourselves as completely excused from entering into the merits of these papers,—which, nevertheless, should be consulted by all who desire to examine the strength of the evidence which Dr Townsend adduces. The best of them is the 10th, which contains a complete series of meteorological observations from the year 1789 inclusive, to August 1822. These observations, which were originally made by Mr Laight of Corlaer's Hook, near the city, on the east side of the Island of Manhattan, are digested partly into tables showing the state of the thermometer and barometer, and also the relative moisture or dryness of the air,—partly into a journal or annuary of remarks on the state of the weather and epidemical constitution for particular years. They bear the marks of being taken and arranged with much care and attention, and cannot fail to be useful in communicating a correct idea of the relation between the weather and its influence on public health.

It is unnecessary to attempt to give any general opinion on the total merits of this work. We have endeavoured to point out to our readers its defects, as a just and scientific treatise, and we have been desirous to do this, not so much in the way of mere opinion, as in that of fair and open examination. Had it not been so deeply tinged throughout with the unhappy bias to which we have alluded, we might hope that the author would yet be stimulated to examine his subject more in the spirit of an observer, and of a person to be instructed, than in that of an adept, whose opinions are already formed, and whose principles are fixed. If these pages should be fortunate enough to attract his attention at the distance of 3000 miles, we would entreat him by the common love of truth with which all of us are actuated, and for the acquisition of which we are all in eager search, that he suffer the remarks which we offer, not only in the spirit of candour, but of sincere regard, to make on him that impression which their principles are calculated to produce. We would entreat him to divest his mind of the very supposition that yellow fever is contagious, and that in his investigation of facts, he assume no principle but what obviously flows from them, and view none through any medium of opinion or hypothesis. We have further to request, that, if in aught we have been betrayed into an undue freedom of animadversion on his opinions, he would ascribe it to the desire which we possess, of seeing every doubtful point either overthrown entirely, or established with those limitations which strict reasoning and the true spirit of philosophy require. Lastly, if Dr Townsend will investigate his subject in the manner we recommend, and will support that doctrine, which we have shown is in its present state a mere hypothesis, on the irrefragable básis of unequivocal and incontestable facts, we shall be the first to announce our conviction of its truth, and the foremost to proclaim the merits of its defender.

II.

1. *A Treatise on Dislocations and on Fractures of the Joints.* By Sir Astley Cooper, Bart. F. R. S. Surgeon to the King, &c. &c. &c. Longman & Co., &c. London, 1822.
2. *Appendix.* pp. 27.
3. *Observations on Fractures of the Neck of the Thigh-Bone, being an Appendix to the Work on Dislocations and Fractures of the Joints.* By Sir Astley Cooper, Bart. F. R. S. Surgeon to the King, &c. &c. &c. Longman & Co., &c. London, 1823.

From the obscure and sometimes uncertain ground of medical pathology, we turn to the more positive and not less useful department of mechanical surgery; and it is impossible to make this transition under better or more skilful guidance than that of Sir Astley Cooper. Every thing done by this eminent surgeon is so strictly regulated by accurate anatomical knowledge, that surgery in his hands may, without exaggeration, be declared to be the most scientific part of the healing art, and to approach more closely to the character of an exact science than any other branch of medicine will ever be capable of doing. To praise the writings or surgical excellence of Sir Astley Cooper, however, is no part of our present purpose; and we shall, for sundry satisfactory reasons to be presently rendered, waive this part of the duty of the journalist. *First*, It is completely unnecessary to eulogize the writings of a surgeon, whose reputation is so justly, so firmly, and so generally established as that of our author. *Secondly*, We are inclined to regard it as very difficult, if not perilous, to attempt a task, in which the desire of doing the subject justice may lead us to exceed the bounds of good taste, which we would not intentionally do. *Thirdly*, we are not without fear, that eulogising an individual like Sir Astley Cooper might be interpreted by many into commendation of ourselves, and that the character of the eulogised would be lost in that of the eulogiser. *Fourthly and lastly*, the best panegyric which can be pronounced is contained in the details of the work before us,—and we therefore proceed, without further preamble, to bestow some consideration on the subjects of our author's Treatise.

The volume now before us is to be regarded as the completion of the Essays formerly published by Mr Cooper, * and comes at present, therefore, under notice in two views chiefly; 1st, as it presents more full or extensive information on the subjects discussed in the Essays; and, 2d, as it communicates information on subjects on which our author had not previously entered. This circumstance furnishes a division not very unnatural for considering the work of our author.

I. On the first of these divisions, then, we begin with stating, that Sir A. Cooper has improved the section on Dislocations in general, and on those of the Hip-joint, by more clear arrangement and more copious information, by accurate accounts of the anatomical peculiarities of the thyroid hole and ischiatic notch; and has rendered the accounts of particular cases of these accidents more instructive, by collecting those from his second Essay, and inserting them under proper heads, with the addition of several new ones, especially in the first Appendix. The details given in these cases, both of symptoms and treatment, cannot fail to be extremely useful to surgical students, and to those provincial surgeons whose limited opportunities of witnessing accidents of this kind in large towns or extensive hospitals, prevent them from acquiring the practical experience of the author.

It is chiefly on the subject of fracture of the neck and upper end of the thigh-bone, however, that Sir A. Cooper has added to the details given in his former publication; and the information which he has communicated on this subject in general, and on its controverted points, is so instructive and important, that we feel it indispensably requisite to enter more fully into their consideration than we should otherwise have done.

The opinions which our author entertains on the rarity of union, when this accident has taken place within the capsular ligament, have involved him in a sort of controversy with an eminent surgeon of the same metropolis; and while this circumstance has been the cause of certain attacks on Sir A. Cooper's professional skill, and on the reputation of the hospitals with which he is connected, and has called forth a rather angry remonstrance, it has been attended with the more beneficial result of a full and elaborate discussion of the point at issue, and, as we are disposed to think, with a confirmation of the principal views of our author. It is by no means our intention, however, to enter into the details of this controversy, nor to consider its merits farther than is intimately connected with its scientific and professional parts; and as we are desirous to shut our eyes on the personal controversies of our brethren, we restrict our present statements and observations closely to the subject of fracture of the neck of the thigh-bone.

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* See Med. and Surg. Journ. vol. xvi. p. 297.

In his former publication, second Essay, our author considered fracture of the neck of thigh-bone as it occurs, 1*st*, within the capsular ligament; 2*d*, without that ligament, when the fracture passes through the root of the neck, or through the great trochanter. In the present work, while the substance of this division is retained, he designates them by the general appellation of fractures of the upper part of the thigh-bone, and divides these into the three following species; 1*st*, that in which the fracture happens through the neck of the bone, entirely within the capsular ligament; 2*d*, that in which the fracture passes through the neck of the thigh-bone at its junction with the great trochanter, which is external to the capsular ligament; and, 3*d*, that in which the fracture passes through the great trochanter, beyond its junction with the neck of the bone. Though the two former only of these fractures properly belong to the neck of the bone, yet as it is certain that the symptoms of all three are so similar, that careless observers, and even surgeons of experience, have been known to consider them as the same, we premise that attention to these distinctions is of the utmost consequence, and absolutely indispensable, in order to understand the pathological principles of our author, and to do justice to his practical inferences.

That there is just ground for these distinctions, and that they are not only pathologically true, but pregnant with useful practical consequences, our author shows at great length, and with much force of evidence, both in the large work, and in two appendices which have been since published. We also learn, that the chief source of the different opinions which have been entertained on the fact of bony union, depends on imperfect or superficial observation of the symptoms and phenomena of these sundry species of injury, and in the want of that undeniable evidence which is given by examination after death, and without which all pathological principles are little better than hypothetical opinions. To satisfy our readers of the justice of these assertions, we shall state, very shortly, the peculiar points on which, in treating this subject, Sir A. Cooper has particularly insisted.

The first species of fracture of the neck of the thigh-bone may be distinguished in three modes; 1*st*, by the symptoms with which it is attended; 2*d*, by the causes by which it is occasioned; and, 3*d*, by its result.

The phenomena which, as diagnostic signs, particularly attend this fracture, are, diminished length and altered direction of the limb. In the course of three or four hours after the accident, the limb becomes one or two inches shorter than the opposite one, its great trochanter is drawn upwards, and the knee, foot and toes, are considerably everted. Both of these changes are ascribed by our author to the power of the muscles operating on the upper part of the bone, which is no longer resisted by the force of the neck propagated from the acetabulum and iliac bone; and the uniformity of their occurrence serves to distinguish the injury not only from the dislocation upwards in the ilium, in which the leg and foot are uniformly inverted, but from the other fractures of the upper end of the thigh-bone, in which this symptom varies according to circumstances. The latter of these symptoms, however, has been said not to be constant; and authors have recorded instances of this fracture, in which the leg, knee and toes, were turned inward. All who are acquainted with the works of Dessault, must remember how much his editor, Bichat, was puzzled to explain this occasional variation of symptoms, and with what accuracy he recorded the proportion of cases in which the limb was inverted, or everted respectively. There is the strongest reason for believing, that the distinction established by our author affords a ready solution of the difficulty, and that eversion of the knee and foot is a uniform result, and therefore a constant symptom of fracture of the neck of the thigh-bone within its capsule.

"It has been asserted," says our author, "that in fractures of the neck of the thigh-bone, the limb is occasionally inverted; but these statements arise from the want of proper discrimination, as it does not occur in the fracture within the capsular ligament, when the muscles have had sufficient time to acquire their permanent contraction; but in a species of fracture of the trochanter major, hereafter to be described, it occasionally occurs." p. 119.

Grating, or *crepitation*, which is such a certain symptom in other fractures, is generally not discovered in this, in consequence of the broken surfaces not being applied to each other; but if the limb be drawn down, so as to cause the broken surface of the lower fragment to touch that of the fragment which is attached to the acetabulum, the grating sound is then heard sometimes, but most certainly if the limb be at the same time rotated inwards.

Before speaking of the proper causes of this species of fracture, it is requisite to notice the kind of subjects in whom it occurs chiefly, and to whom it is confined with a considerable degree of accuracy. Women are much more liable to it than men; and the wards of St Thomas' and Guy's Hospital are rarely without examples of it in the aged female. We have had occasion personally to verify the justice of this remark in the cases most common in the Hospital of this city, having seen only one, of five or six cases, occurring in a man, and that, there was every reason to believe, was to be regarded as fracture without the capsular ligament. In ascertaining the relation of age to the occurrence of this injury, it is requisite to keep in mind the effects of age in different constitutions and in different individuals. Old age, which is always an indefinite term, Sir A. Cooper has justly observed, is as strongly marked at 50 in some persons as at 80 in others; and it depends entirely on the occurrence of those changes which the general feebleness of old age induces on the animal tissues, whether this injury may take place. It rarely occurs, however, under 50; but our author admits that this rule may present exceptions, as he saw an instance of an oblique fracture of the neck of the thighbone in a person at 38, who had an aneurism of the iliac artery, and another, under 50, was shown him by Mr Mayo, (2d Appendix, p. 21.) while he records an instance of dislocation at 62. The interval between 50 and 80 is, on the whole, stated by our author as the time of life most liable to this injury, in consequence of the state of the bone between these periods being such, that the force which previously would produce dislocation, is then attended with fracture of the neck of the bone.

" In London the most frequent source of this accident is from a person, when walking on the edge of the elevated footpath, slipping upon the carriage-pavement; and though it be a descent of only a few inches, yet from its occurring so suddenly and unexpectedy, it produces a fracture of the neck of the thigh-bone. I was informed by a person who had sustained a fracture of this kind, that being at her counter, and suddenly turning to a drawer behind her, some projection in the floor caught her foot, and preventing its turning with the body, the neck of the thigh-bone was fractured. A frequent cause of this accident is a slight fall upon the trochanter major; and I have dwelt particularly on the *slight* causes which produce it, that the young surgeon may be on his guard respecting it, as he might otherwise believe that so important an injury could scarcely be the result of a slight accident, and that excessive violence was necessary to break the neck of the thigh-bone: such an opinion is as liable to be injurious to his reputation, as that of confounding this accident with dislocation." pp. 124. 125.

We come now to the consideration of the point, which our author maintains to be the most important in its influence on our practical measures, and on the subsequent comfort of the patient. We allude to the question, whether the transverse fracture of the neck of the thigh-bone within the capsule is united by ligamentous or osseous matter. Our author, after adverting to the difference of opinion among surgeons on this subject, and to the importance of attending to it in practice, proceeds to state, that in all the examinations which he has made of this injury, not only in early life, but in the course of his subsequent practice, for nearly forty years, he never met with one in which bony union had taken place, or which did not admit of mutual motion of the fragments.

"To deny its possibility," however, he judiciously remarks, "and to maintain that no exception to the general rule can take place, would be presumptuous, especially when we consider the varieties of direction in which a fracture may occur, and the degree of violence by which it may have been produced;—as, for example, when the fracture is through the head of the bone, and there is no separation of the fractured ends; or when the bone is broken without its periosteum, and the reflected ligament which covers its neck being torn; or when it is broken obliquely, partly within, and partly externally to the capsular ligament; but all I wish to be understood to say is, that if it ever does happen, it is an extremely rare occurrence, and that I have not yet met with a single example of it." p. 127.

Now, it appears to us, that this conclusion is stated in the most philosophical and legitimate terms, and is qualified in such a rational manner, that it ought to have exempted Sir A. Cooper from any contradiction of his general principle which was not submitted in the most respectful and friendly style. It is therefore with feelings of considerable pain that we see him obliged to come forward rather in the air of a person who has been wantonly and unjustly attacked, and compelled, as it were, to convert a scientific discussion into a defence of professional skill and vindication of pathological opinions, and a criticism of those of others. And we should regret the more, that the feelings of this excellent man have been so much roused, did we not find that it has been attended with the happy effect of adducing a body of evidence on the point at issue, the strongest and most unequivocal of which the nature of the question admits. It is chiefly the substance of this evidence with which we have any professional concern.

The evidence adduced by our author, either in his large work or in the two short treatises published as appendages to it, may be said to consist of three great divisions. The 1*st* comprehends those pathological results which our author has seen and described in cases of fracture of the neck of the thigh-bone in the human subject; the 2*d* comprehends those results of fracture of the neck of the thigh-bone, which have been described by other observers, and of which prepared specimens are preserved in various collections of the kingdom; and the 3*d* includes those results of fracture of the neck of the thigh-bone, which were observed to take place when the injury was effected experimentally by Sir A. Cooper in the bodies of brute animals.

1*st*. On the first of these subjects the description of the appearances presented by dissection, furnishes the most satisfactory and unequivocal information; and though we cannot bestow much praise on the mere style in which this description is written, it would be injustice to our author and his subject to communicate the matter in any other than his own language.

" The head of the bone remains in the acetabulum attached by the ligamentum teres. There are, upon parts of the head of the bone, very small ossific deposites, covered by the articular cartilage.

" The cervix is sometimes broken directly transversely, at others with obliquity. The cancellated structure of the broken surface of the head of the bone and of the cervix is hollowed by the occasional pressure of its neck attached to the trochanter, and consequent absorption; and this surface is sometimes partially coated with a cartilaginous deposite, which is in some parts studded with slight depositions of ossific matter in spots, so as to fill the cancelli, and produce a structure of a yellow colour upon the bone, which is rendered firm and smooth by friction, as we see in other bones which rub upon each other when their articular cartilages are absorbed. Portions of the head of the bone sometimes are broken off, and these are found either attached by means of ligament, or floating loosely in the joint covered by a ligamentous matter; but these pieces do not act as extraneous bodies, so as to excite inflammation, and thus produce their discharge, any more than those loose portions of bone covered by cartilage, which are found so frequently in the knee, and sometimes in the hip and elbow joints. Some ossific matter is effused on the neck of the bone connected with the trochanter, which is rendered short by an absorbent process; so as in some cases scarcely to project beyond the trochanter. (See Plate.) The appearance of the cancelli of the cervix femoris differs much after this accident, being in some cases scarcely filled, and in others partially covered by a thin pellicle of cartilage, which, receiving afterwards an ossific deposite, puts on a yellower appearance than the original bone, and is smooth on its surface; generally, however, the cancelli are also partially covered by a ligamentous structure.

" The capsular ligament enclosing the head and neck of the bone becomes much thicker than natural, but the synovial membrane which lines it undergoes the greatest change from inflammation, being very much thickened, not only where it lines the capsular ligament, but also upon the neck of the bone, as far as the edge of the fracture.

" Within the articulation a large quantity of serous synovia is found; by which term I mean to express, that the synovia is less mucilaginous, and contains more serum than usual: this fluid, by distending the ligament, separates for a time one portion of bone from the other; it is produced by the inflammatory process, and becomes absorbed when the irritation in the part subsides. I do not know the exact period at which this change takes place, but have seen it in the recent state of the injury. Into this fluid is poured a quantity of ligamentous matter, by the adhesive inflammation excited in the synovial membrane, and flakes of it are found proceeding from its internal surface, uniting it to the edge of the head of the bone. Thus the cavity of the joint becomes distended in part by an increased secretion of synovia, and in part by the solid effusion which the adhesive inflammation produces: the synovial membrane reflected on the cervix femoris is sometimes separated from the fractured portions, so as to form a band from the fractured edge of the cervix to that of the head of the bone; bands also of ligamentous matter pass from the cancellated structure of the cervix to that of the head of the bone, serving to unite, by this flexible material, the one broken portion of bone with the other." pp. 132–134.

From these appearances, which convey a clear idea of the nature of the changes which take place in the neck of the bone after fracture, our author draws the following inference regarding the degree of effort towards union on which the surgeon may calculate:

" It appears then, from this account of the dissection of those whose bodies are examined after having suffered from this fracture, that no ossific union is produced; that nature makes slight attempts for its production upon the neck of the bone, and upon the trochanter major; but scarcely any upon the head of the bone; and that if any union be produced, it is by ligament only." p. 135.

2*d*. Sir A. Cooper is not so unreasonable as to expect the profession to admit this conclusion on the strength of his personal observation only, however extensive that is; and he has been at much pains to ascertain the result of the observations of others, who have directed their attention to this subject. The evidence derived from this source, which constitutes the 2d division, is uniform and conclusive; and it is certain, that wherever examples of this injury have been investigated with the same care, and wherever requisite attention has been given to the distinctions and conditions prescribed by our author, surgeons have obtained the same results. The first, and perhaps the strongest and most decided information which our author adduces of his pathological inferences, is contained in the history of seven dissections of this injury which Dr Colles has described minutely in the Dublin Hospital Reports, and to which five instances of the same kind have since been added. In the first seven, it will be remembered by most of our readers, no union by bone had taken place; and of the other five, Dr Colles expresses himself in the work before us in the following terms:

"Of the fractures within the ligament, not one has made a nearer approach to bony union than that described in the paper alluded to. I must say that I have never yet seen an instance of bony union where the fracture had been within the ligament. We have very many specimens of a disease of the head and neck of the thigh-bone, which is of frequent occurrence amongst our labouring poor. On this subject I have some view of writing a paper for the next volume of the Dublin Hospital Reports, and of endeavouring to show, that in all probability the supposed cases of fracture within the ligament united by bone, were merely instances of this disease." *

Of this disease of the neck of the thigh-bone, we shall probably take occasion to speak; but at present we merely request the attention of the reader to the general fact stated by Dr Colles, and to the mode in which he explains the supposed cases of united fractures of this bone.

To the same effect is the testimony of Dr Monro of Edinburgh, and of Mr Stanley of St Bartholomew's Hospital, London. The former had personally examined two cases after death, in both of which "the broken ends of the neck of the bone were united by a substance like ligament;" and had seen several persons, in whom the accident had occurred without bony union succeeding †; and according to Mr Stanley, the Museum of St Bartholomew's contains twelve specimens of fracture of the neck of the thigh-bone, among which, of six within the capsule, in three there is no union whatever, and in the other three the fragments are united by ligamentous matter. To complete this account of the results of fractures of the neck of the thigh-bone, our author exhibits a tabular view of all the specimens preserved in the collections of the kingdom, which, with the exception of those in the bodies of animals, which

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* Letter, dated February 12th, 1823.

† Letter, dated Feb. 17th, 1823.

belong to the third division of evidence, may be stated in the following order.

" In St Thomas's Collection	5 specimens.
In the College of Surgeons	1 ditto.
In St Bartholomew's	6 ditto.
At Dublin	12 ditto.
In Mr Langstaffe's of Basinghall-street,	6 ditto.
In Mr Bell and Mr Shaw's	6 ditto.
In Mr Brooke's	2 ditto.
In Dr Monro's	2 ditto.
Mr Mayo's Collection	1 ditto.
	41 "

One of these, in the collection of Mr Langstaffe, is justly considered by the author to furnish the most conclusive evidence of which the question admits.

" But the best proof of the difference of a fracture within, and one external to the ligament, is given in a preparation of Mr Langstaffe's, in which a fracture has happened within the ligament, and another external to it; and whilst the latter is seen firmly united, the former has undergone no ossific change." p. 573.

Such are the facts relating to the general occurrence of union as a pathological process, after fractures of the neck of the thigh bone in the human subject; and it is to be kept in mind, that our author has excluded entirely the numerous cases which his long practice has afforded, of ununited fractures of this description in the living subject, and has quoted those only in which the parts were examined at a proper interval after the infliction of the injury. It is scarcely reasonable to demand a greater degree of evidence on any pathological question stated in the qualified manner to which we have already adverted; and it is our own opinion, that it is quite decisive of the point at issue, so far as relates to the mere matter of fact, in the thigh-bone of the human subject. It must, in short, be admitted by all with whom the evidence of dissection has any weight, and who have practically attended to the actual results of this inquiry, that one of three conclusions must follow; either that examples of bony union of fracture of the neck of the thigh-bone, in the human subject, are so rare, that they are at present to be regarded as next to impossible; or, that the inferences of Sir A. Cooper have been misunderstood, in consequence of overlooking the limitations within which he has restricted the occurrence of ligamentous union; or, finally, that the subject is exposed to certain fallacies, which have had the effect of misleading observers, and deceiving them in the formation of their pathological conclusions. What these fallacies are, we shall soon take occasion to show.

3d. To establish, however, more positively the circumstance of ligamentous union, it is merely requisite to attend to the results of those fractures which our author effected experimentally in the bodies of the lower animals. Sir A. informs us, that, after many trials, he succeeded in breaking the neck of the bone in the proper direction in four instances only; yet, in each of these, the process of reparation was limited to the formation of ligamentous substance. In the first case, in which the thigh-bone of a rabbit was the subject of experiment, at the distance of thirty-three days after the injury, the head of the bone was entirely disunited from its neck, which was much shortened, and by its motion had smoothed the broken surface of the head: no ossific change had taken place. In the second, that of a dog, at the distance of thirty-two days after the injury, the joint was lined by adhesive matter of a ligamentous appearance, attached to the head of the bone, the broken surface of which did not seem to be the seat of any ossific process; while the surface of the lower fragment round the capsular ligament and great trochanter was enlarged. These two experiments, it is to be remarked, show that union is not effected, or is ligamentous only, when the fracture is transverse, and consequently, when all circumstances operate to prevent apposition of the broken surfaces. It is evident, however, that if fracture could be effected in such direction that the fragments might *not* be prevented from being applied to each other, it must follow, that want of apposition is not the sole cause of want of bony union, but that there is something in the nature of the parts on which this peculiarity depends. This object is precisely what Sir A. Cooper accomplished in his third experiment, in which the neck and head of the thigh-bone of a dog were broken longitudinally from the trochanter to the articular globe. Twenty-nine days after the injury, a portion of the trochanter which had been broken off, was united by gristle only: the head and neck, which the longitudinal fracture had traversed, were united; but the neck was joined by a larger and firmer quantity of ossific matter than that which joined the separated portions of the head. It is doubtful, our author has remarked, whether, in this kind of injury, ossific union began without the ligament, and was thence propagated to the head, or commenced simultaneously over the whole surface; but he draws the general conclusion, that in longitudinal fracture of the head and neck of the part which is without the ligament, if the fragments be retained in opposition and rest, bony union is produced, though it is slight and imperfect at the head, compared with that which takes place in other bones; and he justly ascribes it in part to the same principle, the operation of which is perceivable in fractures of the knee-pan, olecranon, and similar parts.

"The great difference between the longitudinal and the transverse fracture of the cervix femoris consists in this, that in the longitudinal, as both parts of the head of the bone are remaining in the acetabulum, they are pressed firmly together, and this contact produces their union, even under the slightest ossific action; beside which, the broken head and neck of the bone have sources of nourishment independent of the ligamentum teres; whilst, in the transverse fracture, the actions of the muscles have a constant tendency to separate the portions of bone, and the effusion of synovia and of ligamentous matter into the joint, prevent a continued contact of the fractured surfaces of the bones." p. 139.

It were perhaps to be wished that this experiment had been repeated and varied, for it cannot be doubted that it is extremely instructive, and well adapted to illustrate the process of osseous union, after separation or division of the articular extremities of bones. It is evident, that Sir A. admits bony union to have taken place even in the fragments of the head of the bone, though it was slight, and not so firm as in that of the neck. Now, it is desirable to know how far this *slight* and soft bony union is to be regarded as bony, and to what extent its presence was influenced by the age of the animal on which the experiment was performed. It may be shown, that, in the human subject, fracture of the neck of the thigh-bone depends, as our author has stated, on a condition of the bone not uncommon in old or languid subjects; and we regard it as equally clear, that this circumstance, which operates as a predisposing cause to fracture, has much influence as a preventive cause of union.

We have thus stated the simple facts relating to the actual probability of union in the case of fracture of the neck of the thigh-bone; and have studiously avoided the introduction of any opinions respecting them, that our readers may estimate by the most equitable criterion the strength of the evidence which Sir A. Cooper has adduced. To render this view of the subject more complete and satisfactory, it remains to notice two topics on which our author has in all the three publications before us dwelt at considerable length, but which from this very circumstance lose much of the force which they really possess. We allude, 1*st*, to certain fallacies depending on morbid changes of the neck of the bone, which there is reason to believe have misled several surgeons in forming opinions; and 2*d*, to the circumstances which favour fracture of the bone, and which also prevent the occurrence of osseous union.

It has been repeatedly asserted that instances of union of fracture within the capsular ligament had occurred to surgeons of unquestionable veracity; and it is chiefly on the faith of such assertion that the possibility of union has been maintained. If, however, such cases be minutely inquired into, it will be found that no previous history has been collected; that it is at best doubtful whether the bone had ever been fractured; and that all the evidence adduced is an irregular appearance of fissure in the neck of the bone, which is not unlike an united fracture, and which may deceive a careless or inexperienced observer.

"Two preparations," says Sir A. C. "have been sent to the Royal College of Surgeons in London, as specimens of union by bone of the *cervix femoris;* and as I may be thought prejudiced in favour of the opinion I have advanced, I will give that of an excellent anatomist whose loss we have lately had to deplore. Mr Wilson says, 'I have examined very attentively these two preparations, and *cannot perceive one decisive proof* in either *of the bones having been actually fractured.*'" p. 135.

Now, with regard to the two preparations to which Sir A. alludes, and of which Mr Wilson delivers the opinion just quoted, we know that one is in the person of a girl of five years of age; and although our author takes no notice of this circumstance, we cannot refrain from adverting to the intrinsic evidence furnished by the preparation itself against not merely the fact, but the possibility of fracture of the neck of the thigh-bone. We have no hesitation in stating, that it is an anatomical and physiological impossibility for fracture to occur in the thigh-bone of a person so young. Whoever has attended to the growth of the human bones, is at once aware that at this time of life, and for many years after, neither is the neck of the bone so long as in the adult, nor is the process of ossification completed; but the head and great and small trochanters are still ununited to the body of the bone, or in the state of *epiphyses*, and liable to be detached by a slight force. It may indeed be laid down as an established principle, that the force which in the adult occasions dislocation, and in the aged breaks the neck of the bone, would, in the young subject, previous to the consolidation of the epiphyses, cause the detachment of these processes. It may against this be argued, that the head of the thigh-bone becomes fixed to its shaft at different periods after birth, and consequently that in some instances the neck of the bone may be broken at an earlier period than in others. This we do not deny; but our knowledge of the process of ossification, and of the period at which the head of the thigh-bone becomes firmly fixed to its body, prevents us from believing at present that the neck of that bone had been fractured in the instance now under consideration. In all probability, the appearance which has been taken for fracture, is merely the trace of some irregularity in the process of concretion of the head to the neck, and its union nothing but the natural process of ossification. Of the other preparation we have occasion to know something; and that enables us to say that it may justly be referred to that class of specimens to which we have alluded above. It was obtained from the body of a person in St George's Hospital, the previous history of which was utterly unknown; it was believed, upon what evidence we could never discover, to be an instance of fracture of neck of the thigh-bone, and on that supposition was tendered to the Museum of the College of Surgeons. But the appearances which it presents are exactly similar to those morbid changes described by Sir A. Cooper, and admit of explanation in that manner more satisfactorily than by supposing it to be a united fracture.

We have little doubt that the same explanation is applicable to the case mentioned in the following terms:

" Mr Stanley," says our author, " assistant surgeon to St Bartholomew's Hospital, has kindly shown me a bone, which conveys the idea that the neck of the femur had been broken and united; but it is curious that the neck of the other thigh-bone in the same subject, exhibited appearances nearly similar, only external to the ligament." p. 574.

As this preparation is adduced as one of the examples of united fracture by Mr Earle, it is but justice to Sir A. to show in what manner the appearance is explained.

" The description which our author (Mr Earle) has given of this case, is defective in two very essential points. In the *first* place, he does not say that in one of them the appearance of fracture was external to the capsular ligament, and therefore not applicable to his object. *Secondly,* He does not mention any thing of disease which existed in other joints. I do not mean to deny the possibility of the necks of both thigh-bones in this subject having been fractured, because that point can only be determined by the history of the accident, and by a very careful and accurate examination of several sections of the bones; but I can show that similar effects are produced by disease." p. 38, *Appendix.*

These effects, which were already noticed by the author at p. 575 of his first Appendix, and more particularly in the present, are of two kinds. The first change is described in the following terms.

" The neck of the thigh-bone, in adult persons of middle age, has a close cancellated structure, with considerable thickness of the shell which covers it; but in old subjects, the cancellated structure of the shaft of the bone, which is formed of a coarse net-work, loaded with adipose matter, is often extended into the neck of the bone, and the shell which covers it becomes so thin, that when a section is made through the middle of the head and cervix, it is found diaphanous: of this I have several specimens. As the shell becomes thin, ossific matter is deposited on the upper side of the cervix, opposite the edge of the acetabulum, and often a similar portion at its lower part, and thus the strength of the bone is in some degree preserved: this state may be frequently seen in very old persons. Mr Steel, of Berkhampstead, gave me the thigh-bone of a person thus altered whose age was ninety-three. When the absorption of the neck proceeds faster than the deposit on its surface, the bone breaks from the slightest causes, and this deposit wears so much the appearance of an united fracture, that it might easily be mistaken for it. Before the bone thus alters we sometimes meet with a remarkable buttress shooting up from the shaft of the bone into its head, giving it additional support to that which it receives from the deposit of bone upon its external surface.'—*App. 2d,* pp. 38, 39.

The second kind of change which is liable to lead to error, and which applies immediately to the subject of union, is described in the following terms:

" Old bed-ridden and fat persons, generally females, are often brought into our dissecting room, with some of their bones broken, and more frequently the thigh-bone than any other, in being removed from the grave. If the cervix femoris of such persons be examined, it will be found that the head of the bone is sunken down upon its shaft, and that the neck of the thigh-bone is shortened, so that its head is in contact with the shaft of the bone opposite to the *trochanter minor;* and at the part at which the ligament is inserted into the neck of the bone, the phosphate of lime is absorbed, and a ligamento-cartilaginous substance occupies its place; either extending through the whole neck of the bone, or partially, so that one section exhibits signs of it, and in another it is wanting. The bone, in some cases, is so soft and fragile both in its trochanter and head, that it will scarcely bear the slightest handling; the motion of the thigh-bones in the acetabulum is almost entirely lost, so that the persons could have little use in the lower extremities."—*App. 2d,* pp. 39, 40.

The author then proceeds to state several examples of this morbid change, which had recently fallen under observation in the dissecting-room of St Thomas's Hospital, and closes with the following detail:

" I have had for twenty years in the collection of St Thomas's Hospital, the thigh-bone of an old person, in which the head of the bone had sunken towards its shaft. I have been in the habit of showing this bone twice a year as a specimen how bones sometimes become soft from age and disease, and from the absorption of their phosphate of lime; and I had frequently cut, with a penknife, both its head and its condyles to show this softened state. On sawing through its cervix, the cartilage, deprived of its phosphate of lime, had dried away in several parts, and the appearance was such, that a person ignorant of the change would have declared it to be a fracture; only that in some sections the cartilage had taken different directions, and in some the bone was not yet entirely absorbed."—*Ibid*, p. 41.

These details, if compared with what is stated above by Dr Colles, in the conclusion of his letter to our author, must satisfy every candid and unbiassed inquirer, that the neck of the human thigh-bone, and perhaps other similar parts of the skeleton, are liable to certain morbid changes of structure which are so similar to the traces of a cohering fracture, that they may easily be mistaken by inexperienced observers. We learn, in short, from our author's observations on this subject, two important pathological facts. 1*st*, That the neck of the thigh-bone is liable, in advanced life, or indeed at an early period, to become gradually softened, and in consequence of the constant pressure between the head and trochanter, to become shortened by the process of absorption; 2*d*, that the neck of the thigh-bone is liable, in advanced life, to certain changes of structure, in consequence of which it becomes very brittle, and liable to be broken by a slight impulse.

The last mentioned circumstance is of great importance in explaining, not only the occurrence of this injury at the age at which it commonly happens, but also the difficulty of union at all, and especially by bony matter. It appears, in truth, that the neck of the thigh-bone loses in advanced life, some of its organic properties, and begins to approach to the nature of an inorganic substance, possessing physical properties chiefly. We will not theorize on the cause or nature of this change, which itself is a matter of fact, further than merely to say, that we know from personal observation, that its vascular system is much less extensive and vigorous than during the time, either of adolescence or of adult age. The best and most delicate injections never penetrate either the capsular ligament, or the periosteal covering of this part in old subjects, with the facility, and to the extent to which they do in the persons of the young. The head and neck of the thigh-bone, in truth, appear during advanced life, or in the persons of the aged, to labour under an atrophy or defect of nutrition.

But the operation of this cause is still more powerfully augmented by another circumstance, on which our author has dwelt with peculiar force of physiological illustration. We allude to his anatomical views of the head and neck of the thigh-bone (plate XI.), in which he shows, that those parts are in the natural state supplied with blood by the periosteum, and synovial or reflected portion of the capsule; and that consequently when these tissues are torn or forcibly detached, to which the exceptions in this sort of injury are rare, the round ligament continues to be the only source through which blood is conveyed to the head of the bone.

" When a fracture happens through the neck of the thigh-bone within the capsular ligament, its reflected portion and the periosteum of the neck of the bone are torn through, as well as the ossific vessels of the head of the bone; and the only support which the head of the bone receives, is supplied by the few and minute vessels of the *ligamentum teres*, which are naturally only disposed to produce ligamentous matter. It is not therefore so much to be wondered at, that the head of the bone does not throw out osseous matter, as that its vitality should be supported under such defective nutrition. All bones fractured within the capsular ligaments of joints, and supported only by the portions of reflected ligaments, do not unite by bone as the condyles of the *os humeri*, and the coronoid process of the *ulna*. It appears, then, that there are three causes for non-union of fractured bones; *first*, want of rest; *secondly*, want of adaptation; *thirdly*, the destruction of ossific action in the articular portion of the bone, from its vessels being divided. It is possible that the neck of the thigh-bone might be broken without the strong sheath of the periosteum and reflected ligament which surround it being torn; and, under such circumstances, if they were ever to happen, ossific union might still proceed; but in that case the usual shortening of the limb would not occur." *Note to fig.* 4. *Plate* XI.

The same opinion is expressed at p. 143, in the body of the work.

We have thus, at the risk of being somewhat tedious, endeavoured to give a perspicuous statement of the pathological facts on which Sir A. Cooper founds his opinion of the kind of union of which fracture of the neck of the thigh-bone within the capsule admits, and of the physiological and pathological principles on which his explanation depends. We trust, that, as this is the only true mode of examining the merits of pathological views, our readers will be quite as capable as we, to form their own opinion, and to appreciate the justice of our author's inferences. We have no hesitation in stating our conviction, that he has completely succeeded in establishing the object of his inquiry, and that the present state of our knowledge fully warrants the inference, that fracture of the neck of the thigh-bone within the capsular ligament unites imperfectly, or by ligamentous union only, whatever be the mode of treatment employed. It is not indeed impossible that bony union may take place; but still the facts already collected are at complete variance with this conclusion. The *onus probandi*, in short, rests at present with the opposite party; for, in every case where a majority of instances is in favour of one of two different conclusions, and where the examples which form exceptions are, as in the present case, doubtful, it becomes the duty of the party whose opinion rests on such doubtful exceptions, to search for and adduce them in greater number. When this is accomplished, the conclusion must be changed; and we must then ascribe the ligamentous union to want of rest and want of apposition, and consequently to defect in the resources of art and the skill of the surgeon, and not, as at present, to the peculiar organic nature of the parts injured.

We have room to notice, on this subject, only the opinion of the author regarding the ultimate object or intention, or, what is often preposterously named the final cause of this kind of union; and our reason for adverting to it in this place is, because every inference with regard to ultimate intentions must be merely matter of opinion, and must therefore be received with the allowances and limitations to which they are liable.

"My belief is, that it is wisely ordained by nature, that this, and the other bones I have mentioned—the patella, olecranon, and extremities of the bones within the articulations, should not, as a general rule, unite by bone; for I find, that if the neck of the thigh-bone be shortened, and falls towards the shaft of the bone, the person is unable to move his hip-joint, and becomes, what is vulgarly called, bed-ridden. The cause of this is easily explained; for when the cervix is shortened, the trochanter falls upon the acetabulum, and the play of the thigh-bones becomes destroyed. In an ossific union of the cervix femoris, if such should ever occur, as the neck of the thigh-bone is greatly shortened very soon after the accident, the same effect would be produced, and the play of the limb be destroyed; more especially when the callus, by which the union would be produced, must occupy a considerable part of the acetabulum, and the patient's state would probably be much worse than in that of ligamentous union." *Appendix, No.* 2. pp. 31, 32.

These pathological opinions influence the practice of Sir A. Cooper, who, after detailing all the means that have been, or continue to be in use, concludes with the following acknowledgment:

"Baffled in our various attempts at curing these cases, and finding the patient's health suffer under the trials made to unite them, I should, if I sustained this accident in my own person, direct, that a pillow should be placed under the limb throughout its length, that another should be rolled up under the knee, and that the limb be thus extended for ten days or a fortnight, until the inflammation and pain have subsided. I should then daily rise and sit up in a high chair, in order to prevent a degree of flexion which would be painful. Our hospital patients, treated after this manner, are allowed in a few days to walk with crutches; after a time a stick is substituted for the crutches, and, in a few months, they are able to use the limb without any adventitious support."—pp. 143, 144.

We regret that we cannot stop to notice further our author's account of fractures external to the ligament, nor of those through the trochanter, on both of which much interesting and instructive matter is added.

The investigation of these subjects is followed by that of dislocations of the knee, fractures of the knee-joint, dislocations of the ankle-joint simple and compound, fractures of the tibia and fibula near the joint, and dislocations of the tarsal bones, which are chiefly republished from the second Essay, but with the following changes and additions. Under the head of dislocations of the patella, the dislocation downwards, which is mentioned by some surgeons, is shown to be rupture of the tendon of the *rectus* (p. 183.) The subject of compound fractures of the condyles of the thigh-bone is enlarged and improved by insertion of a dissection (226–235.), and a case of componnd, and one of simple form of the injury. In the subject of simple dislocation of the tibia forwards, a long splint on each side of the limb is recommended, and the management of Dupuytren is noticed (244, 245.) Compound dislocation of the ankle-joint is enlarged by addition of three cases (290–299.) In treating of the cases requiring removal of the ends of the bones, the 9th case is succeeded by an experiment to show the natural process which takes place after removing the extremity of a bone forming a joint (330, 331.) Between the conclusion of this subject and the commencement of that of dislocation of the tarsal bones (353–358.), we find some judicious observations on fractures of the tibia and fibula near the ankle-joint. In treating dislocation of the tarsal bones, our author takes occasion to show the difficulty of reducing the astragalus, and gives a case in which this was attempted without success, first by the surgeon in attendance, and afterwards by the author. The consequence was, that this bone, about twenty-seven days after the accident, became exposed by inflammation and sloughing of the soft parts; and, about forty-six days after this (5th October), it was removed, the parts healed, and the individual recovered, with slight motion at the joint. This is a good instance of the simple dislocation becoming compound. Compound dislocation of the astragalus is, in the present volume, treated of apart from that of the other tarsal bones; and another case is added, in which the bone was entirely removed by Mr Green; and, after various formations of matter, recovery was effected in about fourteen weeks from the date of the injury. The succeeding parts of this division are the same as the former Essay.

II. We come now to the new matter, which embraces the following subjects; dislocations of the lower jaw, of the collar-bone, of the shoulder-joint; fractures near this joint liable to be mistaken for dislocations; structure, dislocations, and fracture of the elbow-joint; dislocations of the wrist, of the carpal bones, of the metacarpal bones, of the fingers, toes, thumb, and of the ribs; and, lastly, on injuries of the spine. Each of these subjects Sir A. Cooper has illustrated with his usual talent of anatomical and pathological research, and has communicated much instructive information. We cannot, however, pretend either to analyze or to abridge these inquiries: for this would do justice neither to author nor reader; and we shall only select a few topics for present consideration, and to show the superior manner in which our author delivers his instructions.

Under the head of dislocation of the clavicle backwards, we find an interesting case, in which the sternal end of this bone was thrown back from its usual position on the œsophagus, and caused much pain and difficulty in swallowing. The material cause of this displacement is said to be distorted spine; but there is some reason to think, that the sternal end of the bone may have been, as often happens, diseased.

" Miss Loffly, of Metfield, Suffolk, had a great deformity, arising from a distorted spine, encreased by an accident which displaced the sternal extremity of the left clavicle, and threw it behind the sternum. The progressive distortion of the spine gradually advanced the scapula, and occasioned the sternal end of the clavicle to project inwards, behind the sternum, so as to press upon the esophagus, and occasion extreme difficulty in deglutition. Her deformity had become excessive, and her emaciation extreme.

" Mr Davie conceived that he should be able to prevent the gradual destruction which the altered position of the clavicle threatened, by removing the sternal extremity of the bone, and the operation which he performed for this purpose was, according to all I can learn, as follows:—

" An incision was made of from two to three inches extent on the sternal extremity of the clavicle, in a line with the axis of that bone; and its surrounding ligamentous connections, as far as he could then reach them, were divided with the saw of Scultetus, (often called Heys,) he sawed through the end of the bone one inch from its articular surface from the sternum, and fearful of doing unnecessary injury with the saw, he introduced a piece of well beaten sole leather under the bone whilst he divided it. When the sawing was completed, he tried to detach the bone, but it still remained connected by its interclavicular ligament, and he was obliged to tear through that ligament by using the handle of the knife as an elevator; and after some time, succeeded in removing the portion of bone which he had separated.

"The wound healed without any untoward occurrence, and the patient was enabled to swallow, from the pressure of the clavicle upon the esophagus being removed.

"She lived six years after the operation, and recovered considerably from her former emaciation. 'Of what she ultimately died,' says Mr Crowfoot, 'I have not learnt.'" pp. 402, 403.

The important subject of dislocations of the shoulder-bone, is treated with much ability and perspicuity. After the necessary anatomical details, Sir A. enumerates three forms of complete dislocation, and a fourth, which is partial only. In the first, downwards and inwards, the bone rests on the axillary margin (inferior costa) of the scapula; in the second, which is forwards under the pectoral muscle, the head of the bone is placed below the middle of the clavicle, and on the sternal side of the coracoid process; in the third, which is backwards, the head may be felt and seen forming a protuberance on the back of the scapula, and near its axillary margin; and in the fourth, the head of the bone rests on the coracoid process of the scapula, at its outer side.

Of these, the dislocation downwards and forwards is by far the most common, and may be easily recognised by a depression at the outer edge of the acromion, the altered direction, increase of length, and loss of power in the arm, and by the head of the bone being actually felt in the axilla. The anatomical peculiarities which result from this dislocation are minutely and correctly described from two cases which Sir A. directed personally. In one of these, which had been dislocated for five weeks, violent attempts had been unsuccessfully used to effect reduction; and the author appears to think that the woman died in consequence of these attempts. In general, the cellular membrane between the muscles and round the joint was distended with extravasated blood, the deltoid muscle was sunk with the head of the bone, the supra-spinatus and infra-spinatus were stretched, the capsular ligament was more or less torn, and, in both cases, the tendon of the subscapular muscle was torn. It is doubtful, we conceive, how far these appearances are constant; for in the one case the dislocation was caused by great violence, sufficient to rupture a tendon; and in the other this might be the result of the violence used in the efforts at extension. The important practical inference from these dissections is thus given:

"It appears from these dissections, that the best direction in which the arm could be extended, is at a right angle with the body, or directly horizontally, rather than obliquely downwards, as the deltoid *supra-spinatus* and *infra-spinatus* muscles are, in this position of the limb, thrown into a relaxed state; and these muscles are, as I have explained, the principal sources of resistance. The biceps is to be relaxed by slightly bending the albow. The arm may be extended directly outwards, in the line between the pectoralis major on the outer side, and the latissimus dorsi and teres major on the inner; but if there be any deviation from this line, it will be better rather to advance the arm, to lessen the power of the pectoralis major. This dissection explains the reason that the arm is sometimes easily reduced soon after the dislocation, by raising it suddenly above the horizontal line, and placing the fingers under the head of the bone, so as to raise it towards the glenoid cavity;—which every tyro knows will sometimes reduce it, because in this position the muscles of opposition are relaxed so as to oppose no resistance to reduction." pp. 423, 424.

Of the four modes of reduction, that by the heel in the arm-pit is recommended as the one to be ordinarily and first attempted, as most convenient, and, on the whole, generally successful in recent cases. In strong muscular subjects, however, where the muscles resist this method, the second, or that by extension and counter-extension, or rather fixation, as it ought to be termed, must be adopted; and if this also be unsuccessful, pullies must be employed, not for the purpose of giving greater force, but to enable the surgeon to continue the force already in use more uniformly and gradually, until, by overcoming the power of the muscles, the bone is made to slip into its place. (427, 430). If these means be aided by the constitutional ones judiciously employed, the bone will rarely resist reduction. The fourth mode, by the knee in the arm-pit, is calculated for recent dislocations in the persons of delicate females, or of the old, relaxed and emaciated. The *ambe*, which our author has mentioned, ought never, according to our observation, to be employed. In all of these, it is of the utmost consequence to take care that the scapula be fixed; for unless this be done, much force is lost, the arm of the patient is most uselessly and painfully stretched, and reduction may be finally unsuccessful. We fear, indeed, that unsuccessful attempts are almost invariably to be ascribed to neglect of this precaution; for we never saw a dislocated shoulder-bone resist reduction, where care was taken to fix the scapula accurately.

The second form of dislocation of the shoulder-joint, which is much less frequent, is distinguished from the former by the acromion being more pointed, the hollow beyond it greater, the arm being shortened, its axis more altered, and, above all, by the bone being distinctly felt below the clavicle. The description of the changes in the structure and situation of parts in an old dislocation, preserved in the Museum of St Thomas's Hospital, will be studied with much interest and advantage. In effecting reduction, the chief peculiarity which demands attention is the direction in which the bone should be drawn, which will be most effectual when slightly downwards; "for if it be drawn horizontally, the head of the bone is pulled directly against the coracoid process of the scapula, and a difficulty is created which may be avoided." (439). In other respects, the same rules must be observed as in reduction of the luxation into the armpit.

The third form of dislocation is the most rare, two cases only having occurred to Sir A. Cooper during the space of thirty-eight years. Its characters are so obvious as to prevent it from being mistaken. Besides the details of one of these two, other three cases are given from correspondents of the author. It does not appear from these details to require any very particular means of reduction, further than rotation of the fore-arm outward, while the usual extension is made. Dislocation *upwards*, described by some surgical writers, is justly stated to happen only when the acromion is broken. It has not been seen by the author. We have seen one instance in the body of a man who fell from a considerable height, but who died in consequence of the shock given to the internal organs.

For dislocation of the shoulder-joint, three forms of fracture near it are liable to be mistaken,—fracture of the acromion, fracture of the neck of the shoulder-blade, and fracture of the neck of the shoulder-bone. To this we add, that we have seen a case, in which the unossified head of the bone in the state of *epiphysis*, was detached by violence, so similar in its appearance to dislocation, that it was in danger of being mistaken for it.

After a correct and distinct account of the anatomical peculiarities of the elbow-joint, Sir A. Cooper enumerates five species of dislocation, as follow:—

1. Both bones (*radius* and *ulna*) are dislocated backwards.
2. Both are dislocated laterally.
3. The ulna is dislocated separately from the radius.
4. The radius alone is dislocated forwards; and
5. The radius is dislocated backwards.

In the first of these, dissection shows the coronoid process of the *ulna* to be thrown into the posterior fossa of the *os humeri*; the olecranon projected at the back part of the elbow one inch and a half above its usual situation; the radius placed behind the external condyle of the humerus, and the humerus thrown forward on the anterior part of the fore-arm, where it forms a large projection. The capsular ligament is torn anteriorly, the coronary ligament may be entire, the *biceps* is slightly, and the *brachialis internus* excessively stretched.

In the lateral dislocation, the coronoid process of the ulna is situate on the back part of the external condyle of the humerus, and is more prominent than in the former case; and the radius projects behind and on the outer side of the humerus, so as to form a hollow above it. In other cases of this dislocation, the ulna has been thrown on the internal condyle of the humerus, where the external condyle of this projects much outwardly.

These dislocations are easily reduced by bending the arm over the knee, and making slight rotation, when the muscles give the two bones their proper direction, and restore them to their original place.

Of the dislocation of the ulna backwards, a dissection is given, from which we learn, "that the olecranon, projected behind the humerus, the radius rested on the external condyle, the coronary and oblique ligaments, and a small part of the interosseous, has been torn. the biceps was thrown backwards, and the brachialis internus was stretched under the extremity of the humerus." (473.)

Of the dislocation of the radius forwards, in which its head rests on the hollow above the external condyle of the humerus, the author has seen six cases; and as four of these had resisted many rational efforts at reduction, he does not disguise the difficulty of the task; but concludes with stating, that the best means of reduction is that proposed by Mr Williams, a pupil of the author, who had seen it reduced by extending the hand only, while the humerus is fixed. (477.) The reason of the difficulty in every other way, and the facility by this, is at once perceived by every one who knows that the first row of carpal bones is connected to the extensive articular surface of the radius, while the ulna is connected to the bones of the hand indirectly, by means of the interosseous ligament only. Force applied to the fore-arm acts on the ulna as well as the radius, whereas force applied to the hand acts on the radius only.

Dislocation of the radius backwards has been seen by our author in the dead subject only, in the person of a man brought to the theatre of St Thomas's during 1821. The head of the bone was thrown behind the outer condyle and the outer side of the humerus. The fore part of the coronary ligament was torn through, the oblique had given way, and the capsular ligament was partially torn, so that the head of the radius was prevented from being further displaced by the fascia of the fore-arm alone. (478.)

In the subject of fractures of the elbow-joint, our author shows, that, when the coronoid process is broken, it is united by ligamentous substance, in consequence of want of ossific nourishment, but that, when the olecranon is broken, if ligamentous union take place, which it generally does, it is in consequence of want of apposition of the fragments. This is well illustrated by experimental fractures, transverse and longitudinal, of this process in the dog, in the former of which the union was ligamentous, but in the latter osseous.

We have only to add, that we have met with a species of dislocation complicated with fracture, which might be expected to have occurred under the extensive observation of Sir A. Cooper, but of which no mention is here made. It consists in fracture of the upper end of the ulna, about an inch and a half or two inches from its extremity, and the fragment being completely displaced, so as to lie on the back of the inner condyle of the humerus. As the person in whom it occurred had been precipitated from a height of at least forty feet on a hard pavement, it was doubtful whether this dislocation and fracture had been caused by direct violence, or by the strong action of the *triceps*. It was reduced without much difficulty; and the man recovered completely, though he required active treatment to obviate the effects of concussion.

The treatment employed and recommended for compound fractures and dislocations of this joint is rational, simple, and very successful. There is nothing in which modern surgery,—what may be called Hunterian surgery, is so superior to the ancient, and nothing in which British surgery has so much the advantage of foreign practice, as in the assiduity and eagerness with which she avails herself of the sanative powers which depend on the process of adhesion. We are certain that no British surgeon will fail to study with delight and interest the treatment employed by Sir Astley and his correspondents, or pupils, in compound dislocations of the ankle and other joints; and we trust that these cases will find their way among foreign surgeons, and be justly esteemed in the French and other Continental hospitals. Three cases of compound fracture of the elbow-joint, which lie before us, furnish the most unequivocal eulogy on this practice that could be imagined; for we venture, without fear of contradiction, to assert, that had these cases occurred thirty years ago, nay, twenty years, in this country, they would have been given to the knife, or treated with poultices; and at this hour in Paris, amputation, or cramming with charpie, would have been the alternative. In the practice of our author, the wound is washed and cleared of extraneous bodies as carefully as possible, but with the caution of disturbing the soft parts as little as may be,—displaced parts are restored to their situation, the edges of the wound are brought accurately together, charpie dipped in the patient's blood, for which application our author has a most natural preference, is laid over it, adhesive plaster is employed to support the parts, and the whole is kept cool by proper applications. The general result appears to be that suppuration is either prevented or is much less extensive than it would otherwise have been, union by adhesion and granulation is promoted, and the patient recovers with a joint the mobility of which is slightly impaired, instead of losing his limb, or being compelled to undergo a long process of suppuration which may threaten life, and finally render amputation indispensable. The 3d case is truly interesting. An old gentleman of 74, had broken the condyles of the humerus, the inner of which with the olecranon also broken, protruded through the skin. The wound was accompanied with considerable hemorrhage, while the comminuted state of the joint allowed it to be twisted in all directions. Yet, under these unfavourable circumstances, the parts were dressed and brought together in the manner already mentioned, and kept in that state for fifteen days; and, though the wound took some time to heal, no suppuration of the joint ever took place, and the bones were united with considerable mobility of the articular ends. Though the last case is not very distinctly described, it appears to belong to that order in which gangrenous inflammation (traumatic gangrene) succeeds to the injury. This unfortunate event, which we have had occasion to see, depends on great contusion and extent of injury in an unhealthy constitution in some instances.

Our limits do not permit us to accompany the author in his enumeration of the subjects of dislocation of the wrist, carpal and metacarpal bones, and fingers and toes, &c.,—but we recommend them most strongly to the attentive perusal of the surgical reader.

The work is concluded with an interesting account of the more common injuries of the spine, in which the effects of violence are considered under the heads of concussion, extravasation, fracture, suppuration, and ulceration. Our personal observation will vouch for the justice of the following remarks.

"It has been generally stated by surgeons, that dislocations of the spinal column frequently occur; but if luxation of the spine ever does happen, it is an injury which is extremely rare, as in the numerous instances which I have seen of violence done to the spine, I have never witnessed a separation of one vertebra from another through the intervertebral substance, without fracture of the articular processes; or, if those processes remain unbroken, without a fracture through the bodies of the vertebræ. Still I would not be understood to deny the possibility of dislocation of the cervical vertebræ, as their articulatory processes are placed more obliquely, than those of the other vertebræ; but from the vicinity of our hospitals to the river, sailors are often brought into them with injuries of the spine, by falls from the yard-arm to the deck; and as there is almost always an opportunity of inspection in these cases, a dislocation must be extremely rare, since I have never met with a single instance of it, they having all proved to be fractures with displacement." pp. 539, 540.

The instance of extravasation within the vertebral canal is good, but we doubt whether the treatment be the best possible in the world. The appearances of the spinal chord and canal, disclosed by dissection, were exactly the same as those observed in the two cases described by Mr Chevalier, in 3d volume of the Transactions of the Medico-Chirurgical Society,* and the symptoms, so far as comparison could be made, were similar. We, therefore, regard the practice rationally suggested by Mr Chevalier as the proper one, since "the resemblance to apoplexy points out the propriety of bleeding as early as possible after the occurrence of the symptoms, in order to prevent an increase of extravasation before constitutional debility takes place." Setons, which were employed in the case before us, are too slow in producing their effects; and mercury, which was used after the consultation, ought on no account to be given.

In the 1st case, in which the dentiform process was broken, the fracture appears to have been consecutive, that is, the result of previous disease of the bone.

Our author has justly observed, that fractures of the spine generally produce displacement of the vertebræ. We are not sure that this displacement is in all cases sufficient to account for the effects on sensation, motion, and the mechanical agents of respiration, which too often follow these injuries; and we would add, as a pathological fact, not useless in its practical consequences,—that the violence which causes fracture, too frequently occasions concussion, rupture of blood-vessels, and extravasation in the chord or into its substance. These effects we have witnessed more than once in dissecting the persons of those who had survived the injury for some days, and in whom the displacement of the bones was trifling; and we rejoice to find they are confirmed by the dissections of our author, who, besides other injuries, mentions, "blood extravasated between the vertebræ and sheath of the chord, and frequently in the chord itself." (p. 556.)

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* Vol. iii. p. 102.

The results of these injuries differ as the fracture is above the third cervical vertebra,—the site of the origin of the phrenic nerve, or below that bone. If above this spot, the accident is generally fatal from interruption not only of the motion of the diaphragm, but of the intercostal muscles, &c.; but when below, as respiration is still continued by the motion of the diaphragm, though that of the intercostals is suspended, death takes place at a period from the injury which varies in different individuals and under different circumstances. The description of these subsequent changes is most instructive, and very faithful to the phenomena; but we trust that our readers will not be displeased that we request them to study this description in the work itself.

In conclusion, we have only to submit a single remark addressed to the author himself. Sir A. has hinted in his preface, that for the accommodation of those who are already in possession of the Essays, if it be their wish, he would publish the new matter of the present volume separately, and in the same form as the first work. Now, though we regard the Essays as a most valuable performance, yet they are to be viewed in the light of a temporary publication only, since the appearance of the work now before us. We, therefore, have to suggest, though with the utmost respect to Sir Astley, that he publish the whole of the present work in the octavo form, with the matter of the two Appendixes either condensed and incorporated, or as at present, at the end of the work. In doing this, we have the vanity to think that if he chance to peruse this article, he may meet with some hints which may not be altogether useless in arranging the matter which he has published on fracture of the neck of the thigh-bone. Of this, however, let the author judge himself. Our reason for recommending a complete edition of the octavo form is, that the work ought to be in the hands of every student and every practitioner of surgery, and it may then be as extensively useful as in its present splendid but bulky and expensive shape, when, of course, it must be accessible to few, and studied by fewer. In the more portable shape of an octavo, it will be the constant companion and book of reference to all, and the valuable lessons which it contains will be diffused through all ranks of the profession.

III.

Pharmacopœia Collegii Regalis Medicorum Londinensis, 1824.
8vo. Londini, 1824. *

THE last avowed edition of the London Pharmacopœia was published in 1809; but impressions, with some slight alterations, were published in 1815 and 1817. The work before us comes forth as a new edition, sanctioned by an order of his Majesty in Council, which "doth strictly require, charge and command, all and singular apothecaries and others, whose business it is to compound medicines and distil waters, or make other extracts," in England, Wales, and Berwick-upon-Tweed, that they "do not compound or make any medicine, or medicinal receipt or prescription, or distil any oil or waters, or make other extracts that are or shall be in the said *Pharmacopœia Collegii Regalis Medicorum Londinensis* mentioned or named, in any other manner or form than is or shall be directed, prescribed and set down, by the said book, and according to the weights and measures that are or shall be therein limited, except it shall be by the special direction or prescription of some learned physician in that behalf;"—a true Highgate oath; which every manufacturing chemist, and every compounder of medicines, thinks himself justified in contravening whenever he thinks fit and reasonable, without fearing to "incur his Majesty's just displeasure," or of being proceeded against, for such their contempt and offences, according to the utmost severity of law. Of this the Royal College are well aware, and it is high time to abandon such empty threats. The observance of the receipts and directions of the Pharmacopœia has always depended, and must always depend, upon their intrinsic excellence and general expediency, the only authority which that of the Edinburgh Pharmacopœia possesses.

We propose to indicate the changes made in the present edition, compared with the impression of the former in 1815, which we have before us; and we shall begin with a subject not at all pharmaceutic, and yet connected with the Pharmacopœia, and highly interesting,—the changes which have taken place in the members of the College, with those entitled *doctiores*.

In 1815, Sir Lucas Pepys was president; in 1824, it is Sir

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* There is another impression in 18mo.

Henry Halford. The censors were, John Mayo, William Heberden, Richard Patrick Satterley, and Edward Nathaniel Bancroft. They are now Richard Powell, Thomas Young, Archibald Billing, and Jeremiah Gladwin Cloves. In this interval 16 Fellows have died, viz. H. R. Reynolds, R. Budd, Sir F. Milman, Sir J. Pennington, J. Littlehales, J. C. Smith, J. Mayo, W. Saunders, M. Baillie, Sir C. Pegge, C. R. Pemberton, R. D. Willis, C. Gower, J. Haworth, G. G. Currey, and R. P. Satterley, or 16 out of 59, or about one in 3.7. During the same period, 36 Licentiates have paid the debt to nature, *viz.* J. Dargent, M. Garthshore, D. Orme, J. H. Sequeira, H. Krohn, J. Lee, James Sims, S. F. Simmons, S. H. Jackson, A. Fothergill, R. Halifax, R. Willan, T. F. Leith, G. Sandeman, T. Dale, R. Bland, A. Thynne, B. Moseley, J. H. Myers, L. Nihell, W. C. Wells, C. H. Parry, A. Marshall, J. S. Robertson, S. de Leon, J. Hemming, T. Bradley, W. Scott, E. G. Clarke, J. Aikin, R. Robertson, R. Dennison, Sir A. Douglas, Sir W. Farquhar, H. Luxmore, A. Marcet, J. Curry, A. Bain, C. Tice, J. Reid, T. Bateman, J. Shaw, S. Fothergill, W. G. Dean, W. Irvine, G. J. Ogilvie, J. Eyre, H. Snow, T. Gray, H. Edgeworth, E. Lockyer, J. Adams, J. Macleod, J. Plenderleath, or 54 out of 152, or about one in 2.8, so that a Licentiate has less chance of long life than a Fellow. In the Pharmacopœia 1815, there were five *Permissi ad artem obstetriciam exercendum;* there is none in the present, but we do not know whether this class is extinct, or only omitted. Of the *Permissi extra urbem*, 17 have died, out of 34, or one half, tending to show that the life of a country practitioner is not equal to that of a town physician.

In 1815 there were 59 Fellows, 152 Licentiates, and 34 Extra-Licentiates. In 1824 there are 88 Fellows, 222 Licentiates, and 42 Extra-Licentiates, being an increase of nearly one half in the Fellows, of 0.4 in the Licentiates, and about 0.2 in the Extra-Licentiates—from which it may be inferred, that there is a greater desire of being admitted among the Fellows than formerly. By the way, we are happy to see that the Junior Fellow is a Graduate of this University. Has the London College broke through their excluding bye-laws? or did Dr Southey owe this honourable distinction to his having been previously appointed, or to his being about to be appointed, physician to the King?

Of the Pharmacopœia itself, the general arrangement remains the same. In the Materia Medica there are not many alterations. The new articles introduced are, *acidum aceticum fortius*, strong pyrolignic acid having the Sp. gr. 1.046, and 100 grains requiring 87 of subcarbonate of soda to saturate them; *Antimonii vitrum*, vitrified sulphuretted oxide of antimony; *Bismuthum; Marmor album; Acidum critricum; Magnesiæ subcarbonas*, and *Potassæ sulphas*, formerly only among the articles to be prepared by the apothecary, are now permitted to be purchased as articles of commerce from the manufacturing chemist. *Colchici, conici et digitalis semina*, the seeds of three narcotic vegetables, of which the leaves or herb were formerly inserted. *Daturæ folia et semina*, another narcotic, both herb and seeds, for the first time officinal. *Lactuca*, for its inspissated expressed juice. *Cubeba*, the dried berries used in gonorrhœa. *Helenium*, elecampane root, as an ingredient in the *Confectio piperis nigri*; *Krameriæ radix*, rhatany root, an astringent, and *Tiglii oleum*, croton oil.

The following articles are rejected; *Aloes vulgaris extractum*, Barbadoes aloes, and *Vinum*, wine, which they do not employ, although they have retained *Vina* as the title of a section of the preparations, and *Vinum* as the title of five preparations which contain not a drop of wine, which must amuse our friends on the other side of the Channel.

In other respects, the changes are very trifling. They have omitted *Acidum aceticum impurum* as the scientific name of *Acetum; Arsenici oxydum* has become *Arsenicum album*, with *Acidum arseniosum* as its scientific name. Camphor is described more correctly, as prepared by sublimation instead of distillation; to colocynth and elaterium apples, the term *pepones* is with propriety substituted for *poma; Kino* is said, upon the authority of the Encyclopedie Methodique, to be the product of the *Pterocarpus erinacea;* we believe that at present used in Britain is East Indian, and the extract of the *Nauclea Gambir: Resina nigra* has been changed into *Pix nigra*. They still designate the *Heracleum gummiferum* as the name of Gum Ammoniac, merely because that plant was raised from seeds found adhering to the Gum, although by inspecting the delineation of the real plant in Jackson's Morocco, it is almost certainly produced by a different plant probably a *ferula;* and they still erroneously require the strong sulphuric acid to have the specific gravity of 1.85, although it be ascertained that it cannot have this high gravity unless it be adulterated, and although the other Colleges have correctly given 1.845.

We now proceed to point out the alterations in the second part, describing the *Præparata et Composita*.

Acida.—Benzoic acid is now directed to be prepared by sublimation, instead of solution in lime-water and precipitation. The strength of the acids is estimated by subcarbonate of soda instead of chalk. To saturate 124 of the subcarbonate, 100 grains of muriatic acid are required, and of nitric acid 212. A formula is introduced for the preparation of tartaric acid from cream of tartar by lime, and sulphuric acid.

Alkalia et eorum Sales.—In preparing the *Liquor ammoniæ*, the receiver is ordered to be kept cool, not exceeding 50°. For the definite quantity of acetic acid, formerly prescribed in preparing *Liquor ammoniæ acetatis*, this modifying phrase, *vel quantum satis sit*, is added. *Potassæ acetas* is directed to be prepared with pyrolignic acid diluted with water, instead of acetic acid, and the solution, after saturation, to be evaporated to successive pellicles, which are to be removed and dried upon bibulous paper. The carbonates of potass and of soda are now prepared by transmitting through a solution of the subcarbonate a stream of carbonic acid gas and evaporating at a temperature not exceeding 120°, instead of the process with subcarbonate of ammonia.

Terræ et eorum Sales. No change.

Metalla et eorum Sales.—The process for preparing the *Antimonium tartarizatum*, introduced into last Pharmacopœia, is again abandoned, and that by boiling glass of antimony in a solution of cream of tartar substituted. In the former edition, the solution of tartar emetic in diluted wine was called *Liquor antimonii tartarizati*, and in the present, by a strange inconsistency, they have restored the old title, *Vinum a. t.*, while they have rejected the wine and substituted proof spirit. *Subnitras bismuthi*, prepared by precipitating a solution of the nitrate by water, is new. *Ferrum ammoniatum* is prepared by subliming with sal ammoniac an extemporaneous muriate of iron, instead of the subcarbonate. The process for *Ferrum tartarizatum* is modified, chiefly in continuing the exposure of the iron and cream of tartar in a moist state to the air for a longer period, and in dissolving the triple salt in water, filtering and evaporating to dryness. *Vinum Ferri* (without wine) is prepared by exposing iron one drachm, and cream of tartar six drachms, constantly moistened, to the air for six weeks, drying, dissolving in 36 fluid ounces of distilled water, filtering, and adding 20 fl. oz. of proof spirit. *Hydrargyrum Purificatum* is distilled in an iron retort, without the addition of iron-filings. *Hydrargyri Submurias* is prepared by a new process.

"Take of Purified quicksilver, four pounds.
Sulphuric acid, thirty ounces.
Muriate of soda, one pound and a half.
Muriate of ammonia, eight ounces.

Boil two pounds of the quicksilver with the sulphuric acid in a glass vessel, until the sulphate of quicksilver formed be exsiccated. When this has cooled, triturate it in an earthen mortar with the other two pounds of quicksilver, until they be intimately mixed; then add the muriate of soda, triturate again, until no globules of mercury can be seen, and sublime. Triturate the sublimate into a most subtile powder, pass it through a sieve, add the muriate of ammonia previously dissolved in a gallon of water, and mix them thoroughly. Set aside, until the powder subside. Pour off the liquor, and wash the powder repeatedly with boiling distilled water, until no precipitate is formed, on the addition of *Liquor ammoniæ*. Lastly, reduce to a very subtile powder in the way chalk is prepared." Sugar of lead is now called *Plumbi Acetas*, instead of *Super-acetas*, and is prepared with diluted pyrolignic acid. *Zinci oxydum* is prepared by precipitating a solution of the sulphate by the *Liquor ammoniæ*.

Sulphurea, *Vegetabilia*, *Olea expressa*, *Olea destillata*. No change.

Aqua destillata. Two modes of preparing the volatile oils of Cinnamon, Peppermint, Mint, and Pennyroyal, are given; one, as before, from the drug in substance; and the other, we believe more commonly in use, by distilling water with a proportion of volatile oil; five scruples of oil of cinnamon being held equivalent to a pound of the bark; and three drachms of the oil of peppermint, mint, and pennyroyal, to a pound and a half of the dried herb; the quantity of the product being in all cases a gallon.

Infusa. No change, except the addition of the epithet *Compositum* to the titles of the *Infusum Lini* and *Infusum Rosæ*.

Mucilagines. *Decocta*. No change.

Extracta. Some of the extracts are directed to be prepared in greater quantity; and two new extracts are introduced. *Extractum Stramonii*, prepared by digesting one pound of the seeds in a gallon of boiling water for 24 hours; then bruising the seeds, and returning them into the liquor, boiling down to four pints, straining while hot, and finally evaporating to a proper consistence. This furnishes a powerful narcotic extract. The other, the *Extractum Lactucæ*, from its injudicious mode of preparation, will be nearly inert. It is directed to be prepared from a pound of the fresh herb of garden lettuce, bruised in a stone mortar with a little water; the juice expressed and evaporated, without defæcation, to a proper consistence. This extract, it is obvious, will contain the whole principles in the common sap, and in the proper juice; the former, by far the most abundant, consisting of saccharine and mucilaginous principles, and the latter being also nearly inert, except when the plant is shot, when it contains an anodyne principle. But as the College do not specify that shot lettuce is to be employed, the extract may be without any virtue, and even if prepared from shot lettuce, it will be very inert in comparison with the inspissated juice obtained by wounding the plant, either dried entire with all its caoutchouc, or in the manner recommended by Mr Probart.

Misturæ. No change.

Spiritus. Spiritus Cinamoni is greatly improved by distilling it from the oil, of which five scruples by weight are distilled with four pints and a half of rectified spirit, and a little water to prevent empyreuma, and a gallon drawn off. The following spirits are directed to be prepared with the oil; peppermint and mint six scruples and a half, and pennyroyal seven scruples, with four pints and a half of rectified spirit; rosemary one ounce with a gallon of spirit and some water, and of each a gallon is to be drawn off.

Spiritus Colchici Ammoniatus is new. It is an infusion of two ounces of bruised colchicum seeds in a pint of *spiritus ammoniæ aromaticus.*

Tincturæ. No change.

Ætherea. Rectified ether is directed to be washed after rectification, twelve fluid ounces being shaken with nine fluid ounces of water.

Vina. In all the officinal wines, four parts of proof spirit and six of water are substituted for wine. *Vinum colchici* is added, made with one pound of the fresh root sliced, four fluid ounces of proof spirit and eight fluid ounces of water.

Acetica. No change; still made with distilled vinegar; pyrolignic acid duly diluted would be preferable.

Mellita. No change.

Syrupi. Syrup of sarsaparilla is introduced.

Confectiones. Confectio Piperis Nigri is added. It is compounded of black pepper, and elecampane root, each one pound, fennel seeds three pounds, honey and sugar two pounds. It will be sufficiently pungent, but we do not think the ingredients well chosen.

Pulveres. No change.

Pilulæ. The compound gamboge pill is altered. It now consists of one drachm of gamboge, a drachm and a half of extract of aloes, half a drachm of ginger, and two drachms of soap.

In the *Pilulæ Hydrargyri Submuriatis Compositæ*, the active ingredients remain the same, but they are desired to be brought to a proper consistency with rectified spirit of wine, instead of mucilage. This is surely a pharmaceutical error; for the mass will, from the great volatility of the spirit, become almost immediately hard, and the pills themselves will be very apt to pass through the alimentary canal without any action.

Præparata ex Animalibus. No change.

Emplastra. To the former ingredients of the Cumin plaster, a fluid ounce and a half of olive oil, and as much water, are added. In making the opiate plaster, half a pint of water is added, which is to be boiled slowly away. To the pitch plaster two fluid ounces of olive oil, and as much water, are added, which will render it less hard.

Cerata, *Unguenta*, *Linimenta*, and *Cataplasmata*. No change.

The changes in nomenclature are few, and although of no great importance, are not injudicious. The following Table exhibits them.

NOMINA PRIORA.	NOMINA NOVA.
Acidum aceticum.	Acidum aceticum dilutum.
Arsenici Oxydum.	Arsenicum album.
———————— sublimatum.	———— album sublimatum.
Calumbæ *Radix.*	Calumba.
Ceratum Lyttæ.	Ceratum Cantharidis.
———— Plumbi Superacetatis.	——— Plumbi Acetatis.
Cucumis Colocynthis, *Pomorum pulpa.*	Cucumis Colocynthis, *Peponum Pulpa.*
Elaterii *Poma.*	Elaterii *Pepones.*
Elettaria Cardamomum.	Matonia Cardamomum.
Emplastrum Lyttæ.	Emplastrum Cantharidis.
Infusum Lini.	Infusum Lini compositum.
———- Rosæ.	———- Rosæ compositum.
———- Sennæ.	———- Sennæ compositum.
Lapis calcarius.	Marmor album.
Liquor Antimonii tartarizati.	Vinum Antimonii tartarizati.
Lytta.	Cantharis.
——- vesicatoria.	——— vesicatoria.
Magnesiæ Carbonas.	Magnesiæ Subcarbonas.
Oxydum Arsenici album.	Acidum arseniosum.
Pix arida.	Pix abietina.
Plumbi Superacetas.	Plumbi Acetas.
Resina nigra.	Pix nigra.
Tinctura Lyttæ.	Tinctura Cantharidis.
Unguentum Lyttæ.	Unguentum Cantharidis.
————- Resinæ nigræ.	————- Picis nigræ.

From this analysis, in which we have attempted to notice every alteration of any, even the slightest, consequence, our readers will be fully sensible of the very small progress in Materia Medica and Pharmacy during the last twenty-five years, considered as sufficiently established by the Royal College of Physicians to receive their sanction. Although various other additions might have been made of articles which are in common use, such as the Hydrocyanic Acid, the Sulphat of Kina, &c.; and although several articles, now entirely neglected, might have been struck out, we are not, upon the whole, disposed to quarrel with the principle of being rather slow in adopting real improvements, than hasty in embracing every plausible alteration. In the Nomenclature also, scarcely any change has been made; and the College has certainly not subjected itself to any censure on account of precipitancy. We frankly confess, that, in regard to this subject, we have entirely changed our opinion; and take this opportunity of making the *amende honorable* to Dr Bostock, whose judgment we formerly (Vol. VII. p. 367.) strenuously controverted. Experience has shown, that the use of a scientific nomenclature in Pharmacology has no tendency to render it either permanent or instructive. To prove this, it is sufficient to compare the Nomenclature of the British Colleges with that of any of the modern Continental Pharmacopœias. For example, Corrosive Sublimate, after having been successively named, *Murias hydrargyri corrosivus*, *Murias hydr. oxydatus præparatus*, *Hydrargyrum muriaticum corrosivum*, *Murias hydrargyri*, *Murias hydrarg. superoxygenatus*, *Hydrargyri oxymurias*, has now received, in the French Pharmacopœia, *Deuto-chloruretum hydrargyri*, as its scientific name; and if we adhere strictly to the doctrine upon which it proceeds, the solution of this substance in water must be considered as a mixture of chlorate of mercury and hydrochlorate of mercury, to express which in a name would not be easy or convenient; and, after all, the next revolution in the science of Chemistry, and we hope to live to see more than one, will render the whole trouble useless, and worse than useless. The same fluctuation is inevitable, from connecting the names of the simples with the scientific progress of the kingdoms of nature from which they are derived. *Cantharides*, and Spanish flies, are terms understood in every part of the world. The Edinburgh College gave them the name of *Melöe*, from a new entomological arrangement; the London College, adopting a more improved system, called the genus *Lytta*, and now have reverted, on the authority of Lattreille, to the original *Cantharis*, which we hope may be left to these useful insects in all time to come. In the scientific nomenclature of vegetables, there is the same incessant fluctuation. *Zingiber*, became *Amomum*, and is now again *Zingiber*; *Cardamomum* has been *Amomum repens*, *Amomum cardamomum*, *Elettaria*, and *Matonia*;

but the substance remains always the same. Other substances, such as *Kino*, which are derived from more than one vegetable, are referred to one only, and by each of the British Colleges to a different plant, and by none of them to that which furnishes the variety commonly occurring in commerce.

But the subject would lead us into too great length, and we shall terminate with the words of the venerable Hufeland. *

" Le mal est urgent, et menace de s'étendre de jour en jour. Il est tems d'y mettre un terme. Le remède en est facile; il ne tient qu'à nous d'en faire usage, et j'ai cru devoir prendre la parole et de traiter la chose publiquement, afin de solliciter et d'obtenir l'attention et l'assistance de mes Collégues." p. iv.

" Il ne faut pour cet effet que *la réunion de tous les Médecins praticiens* et *académiciens, et de leur part l'engagement inviolable, de ne se servir dans leurs ordonnances que des anciens termes, et d'aucune autre langue que de la latine.*" p. xx.

IV.

1. *Medical Jurisprudence.* By J. A. PARIS, M. D. and J. S. M. FONBLANQUE, Esq.—*continued.*
2. *Leçons faisant partie du Cours de Médecine-Légale de M. Orfila, Professeur à la Faculté de Paris.* 1821. 8vo. pp. 503. avec 22 planches coloriées.

ACCORDING to our intention, announced while reviewing the work of Dr Paris and Mr Fonblanque in our Number for last October, we shall now present our readers with some account of that part of their Treatise which is occupied with the subject of Death by Poisoning. And to this account we shall add a few remarks on the Outlines of Professor Orfila's Lectures on the same subject; a work which has been more than two years before the public, but does not appear to have been read in Britain, and by Dr Paris in particular, with the interest and attention it deserves.

Of all the departments of medical knowledge, none has arrived more rapidly at the dignity of a distinct science, than Toxicology. As a topic of philosophical inquiry, it can scarcely be said to have been pursued earlier than the middle of last century. Nor was it investigated systematically for some time afterwards; when at last the labours of Fontana, the ingenuity of Foderé and Brodie, and the indefatigable industry of Orfila, conferred on it in a few years its rank as a science. The same causes which have accelerated its progress, will probably ensure its stability and future advancement. On the one hand, few of the medical sciences have been founded so purely on the immutable basis of observation and experiment; in none have fact and hypothesis been kept so carefully apart from each other; and in few have the leading doctrines been so often and so amply confirmed by fresh investigation. On the other hand, few objects of medical pursuit possess so many attractions for all classes of students. From it the physiologist borrows several of his most powerful and accurate instruments of research; from it may be deduced some of the soundest and most useful doctrines in therapeutics; and from toxicology does the medical jurist in a peculiar manner derive the most essential aid in the fulfilment of his complicated and important duties. It is in the last point of view only that we propose to consider it at present. In reference to medical jurisprudence it is becoming every day more and more an object of interest. For the necessary result of the attention it has lately received from the scientific, has been an increased knowledge of it among the vulgar and unprofessional; and consequently, means more varied and dexterous are now resorted to for the purpose of administering poison. It is to be wished that, from this serious and increasing drawback on the advantages of extended knowledge, those on whom society depends in a great measure for protection, would derive an argument to induce them to throw aside the indifference at present manifested towards the study of toxicology, and prepare themselves better for the inquiries they are often called on to undertake.

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* Addresse à tous les Médecins sur la Necessité de Conserver le Nom officinal des Médicamens, par Hufeland. 12mo. Berlin, 1821.

The chief difficulty to be encountered in the study of toxicology, is its perplexed, inaccurate, and unphilosophical classification. This inconvenience, without doubt, is in a great measure inseparable from it; but at the same time authors do not appear so successful as they might have been in diminishing it. The first feasible attempt to classify poisons was made in 1813 by Professor Foderé, now of Strasbourg; who divided them into six classes, the Septics, Narcotics, Narcotico-acrids, Acrids, Corrosives, and Astringents. The same arrangement was adopted by Professor Orfila in both editions of his great work on Toxicology, published in 1814 and 1818. It is also this arrangement which has been preferred by Dr Paris and Mr Fonblanque; but Dr Paris has likewise attempted to show, that a superior system might be founded on the mode in which the individual poisons act on the animal economy. We have already stated our opinion on his proposed classification; we have endeavoured to prove, that in every class and order its fundamental principle is essentially departed from; and it would require but little argument to show that it is also clumsy, if not useless, when applied to practice. Of this, indeed, our author seems to be fully aware; for although he constructed the arrangement long ago, he has not followed it in the present work.

But the classification, which Dr Paris and Mr Fonblanque have actually adopted, is scarcely less objectionable.[*] Of the substances contained in their first class, or corrosives, not one-third possess the property of burning or corroding the tissue to which they are applied. The second class, or the astringents, comprehends but one genus, the preparations of lead. These preparations, it is true, produce "unrelenting constriction," or spasm of the intestines; but there is as little reason for calling them astringents on that account, as there would be for arranging nux vomica and some others in such a class, because they excite unrelenting constriction of the muscles of voluntary motion. The third class, or acrids, do not produce a single symptom which can distinguish them from the corrosives. And with regard to the class of septics, it appears very questionable, whether any poison can produce such a dissolution of the humours as the name implies. The distribution proposed by Orfila in the Outlines of his lectures[†] is much more perfect. He rejects the astringent poisons altogether, and combines the acrids and corrosives together under the name of Irritants. Had he likewise discarded altogether the septic poisons, he would have rendered the classification as perfect as it probably will ever be made. The sulphuretted hydrogen, the poison of venomous animals, and that of putrid animal matter, which are commonly reckoned and arranged as septics or putrefiants, have no such effect as their name indicates; and an appropriate place may easily be found for them in one of the three remaining classes, the *Irritants*, *Narcotics*, and *Narcotico-acrids*.

The class of Irritants should include all poisons, whose predominating symptoms are those of irritation; the Narcotics those which produce stupor, delirium, and other affections of the brain and nervous system;[‡] and the Narcotico-acrids those which cause sometimes narcotism, sometimes irritation, sometimes both together. We see no sound reason why Dr Paris and Professor Orfila should doubt the propriety of the name and class of narcotico-acrids.

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[*] See the Number for October 1823, p. 626.

[†] Also followed in his "Instructions for the Treatment of the Asphyxiated and Poisoned," &c.

[‡] It is almost unnecessary to observe, that the word Narcotic, as now used by toxicologists and others, differs very widely from its original signification. As derived from its Greek root, a narcotic poison should mean a poison which stupefies; but the same term is now very generally used to embrace all poisons which produce nervous symptoms; and in such a sense it is employed above.

It is needless to caution the critical reader, that each of the classes now described must contain some individual poisons, which might be arranged, with almost equal reason, in one of the other two. No system of arrangement in any science is free from this disadvantage: each class must include objects, by which it passes insensibly into the rest.

No leading principle exists to direct us in constructing the orders of the foregoing classification. Probably some modification of those formed by Orfila in his lectures, will be found as correct as any that can be attempted in the present state of our knowledge. Thus the Irritants may be arranged in six orders; 1. The concentrated mineral acids, phosphorus, iodine and chlorine; 2. The fixed alkalis, their carbonates and nitrates; 3. Ammonia and its salts, and the alkaline sulphurets; 4. The metallic compounds; 5. The vegetable acrids and cantharides; 6. The acrid gases. The Narcotics might consist of two orders only, the Vegetable Narcotics, and the Narcotic Gases. The Narcotic-acrids include six very artificial and precarious orders; 1. Those which cause delirium as their predominating symptom, of which belladonna is the type; 2. Those which cause pure tetanus, such as nux vomica; 3. Those which cause tetanus and coma conjunctly, such as the cocculus indicus; 4. The poisonous mushrooms; 5. Poisonous animals; 6. Spurred rye.

The Toxicological part of the work of Dr Paris and Mr Fonblanque is prefaced by an abstract of the laws of England against the crime of poisoning. They next give a short and imperfect, but interesting view of the literary history of poisons; they then proceed to treat of *slow*, *accumulative* and *consecutive* poisoning; and conclude the introductory part of the subject with an investigation of the several questions in Toxicology, which may occur during criminal trials. These questions are five in number: 1. Whether the symptoms caused by poisons may be likewise caused by natural disease? 2. Whether the morbid appearances they produce may be also produced by natural causes? 3. Whether rapid putrefaction of the body favours the presumption of poisoning? 4. Whether the fact of poison not being discoverable in the alimentary canal argues against an accusation of poisoning? And 5. Whether any evidence can be deduced from the effects of the visceral contents on animals? This concludes their account of General Poisoning, which our readers will readily perceive is discussed in a very irregular manner. They next proceed to consider the poisons individually, in the order we have related. Under the head of each poison, if it is one of importance, they give, *first*, its chemical history; *secondly*, its symptoms; *thirdly*, its mode of action; *fourthly*, the morbid appearances it produces; and, *lastly*, its tests in its pure and in its mixed state.

The plan pursued by Professor Orfila is somewhat different, and much more precise and philosophical. He begins with a detailed account of the individual poisons, treating under each head, *first*, The tests for the poisons when solid, when fluid, when *mixed* with other solids, or with other fluids, when *combined* with other substances, and when mingled with vomited or alimentary matters; *secondly*, Its symptoms; *thirdly*, The organic changes it gives rise to; *fourthly*, Its mode of action. Having discussed all the known poisons in succession, he presents us with a very full treatise on General Poisoning. Here he shows how the medical jurist is to determine, *first*, Whether the crime of poisoning has been committed; namely, by considering the symptoms, the morbid appearances, the natural diseases which imitate these symptoms and appearances, and the effects of the suspected matters on animals; and, *secondly*, What particular poison has been employed. These elaborate inquiries, which occupy an entire fifth part of the book, are succeeded by a few remarks on slow poisoning, and some other collateral topics of less moment; and he concludes with an Appendix of Reports on suspected cases, and another on Food relatively to Medical Police.

It is evidently quite impossible to give, in the short limits of a review, even an abstract of all the subjects embraced by the two works. Our readers will be probably satisfied with a few specimens of the method and execution of each author; and, under this conviction, we shall first examine that part of Dr Paris and Mr Fonblanque's treatise, which discusses the different questions that may occur on General Poisoning; then we shall compare with it the treatise on the same subject by Orfila; and lastly, we shall attempt a critical analysis of Dr Paris's observations on the most important individual poisons.

The first question which may occur on criminal trials, relates to the possibility of natural disease producing symptoms similar to those produced by poisons. On this subject most toxicologists have expressed the same opinion; namely, that little or no dependence can be placed, in proof of poisoning, on the symptoms alone. The opinion appears most accurate, so long as it is applied to the generic symptoms produced by the irritants on the one hand, or the narcotics on the other. But almost every modern author, even Professor Orfila, has erred, by applying it too universally in particular instances, and extending it to the specific symptoms of every individual poison. "The physician who, judging from the symptoms and morbid appearances only, should *affirm* that a person had been poisoned, would be blameable."—"He would be still more culpable if he were to *depone* to this effect without having actually detected poison." *Orfila*, 360, 361.

Now, we apprehend there are cases, and these not very uncommon, in which an irrefragable opinion in favour of poisoning may rest on the symptoms alone. And in proof of what is stated, we may notice shortly the occasional effects of *nux vomica*, of *oxalic acid*, and of *arsenic*. If within half an hour after swallowing a bitter drug, or suspected article of food, a person were to be seized with pain and rigidity of the joints, gradually increasing in paroxysms till the whole body became convulsed with tetanus, and death should take place within one or two hours, the person preserving sensibility to the last,—could any toxicologist doubt that he had been poisoned, and that the poison was nux vomica, or one of the same genus of plants? If, immediately after taking a sour medicine or fluid, a person were seized with burning pain in the stomach, speedy, violent, incessant vomiting, followed by loss of the pulse, tetanic convulsions, insensibility, and death within an hour, could any one doubt that oxalic acid had been taken? Or if, to the usual symptoms of poisoning by the irritants, were added signs of irritation throughout the whole mucous membranes of the body, succeeded or attended by partial palsy or epilepsy, or both, could any one doubt that his patient had swallowed arsenic? For where is the natural disease, which can imitate such examples of poisoning? Other substances might be added to those now mentioned: corrosive sublimate, lead, cantharides, hydrocyanic acid, even opium and alcohol *occasionally* cause symptoms, which, if carefully observed and weighed by a judicious toxicologist, cannot be confounded with the effects of any natural disease whatsoever. While endeavouring to check the rashness which characterized the opinions of the medical witnesses of the last generation, authors on toxicology have gone a step farther than was necessary in their admonitions. Hence, it need not surprise us that medical witnesses now-a-days are apt to err by excessive caution. Throughout our criminal records, examples might easily be found of witnesses having committed serious oversights by thus acting under the impulse of timidity, and the sanction of respected names.

The question now before us does not appear to have been considered by Dr Paris and his coadjutor with all the attention its importance merits. The only disease they notice, which may be confounded with poisoning by the irritants, is cholera. Their remarks on this disorder, and the diagnosis between it and acrid poisoning, are exceedingly judicious. They observe, that although the leading characters of both affections are the same, (as indeed they must be, seeing the affections themselves are in essence the same), yet a sound diagnosis may generally be derived from the collateral circumstances, such as "the season of the year, the prevailing epidemics, the age and predisposition of the patient, his habit with respect to diet," the progress of the case, and certain subsidiary symptoms, if they may be so called, which often attend poisoning, but seldom or never cholera. But Dr Paris must be aware, and might therefore have informed his readers, that other diseases also may, in certain circumstances, be confounded with the effects of the irritating poisons. Orfila has enumerated the six following,—spontaneous erosion of the stomach, acute inflammation of that organ, the iliac passion, strangulated hernia, peritonitis and melæna. To these we would also add, the gastritis erythematica of Dr Cullen; a disease by no means common indeed, but which, when it does occur, resembles more a case of slow acrid poisoning than any other disorder, perhaps even than cholera. For the means of distinguishing these different affections from true poisoning, we must refer to the Lectures of Professor Orfila, pp. 342–360.

As to poisoning by the narcotics, its symptoms are so closely imitated by several nervous diseases, such as apoplexy, epilepsy, hysteria, concussion of the brain, that, except with regard to a few peculiar poisons, the diagnosis is almost or absolutely impossible. Dr Paris and Mr Fonblanque have discussed this curious and important topic in a single paragraph of 13 lines; nor can we venture to go along with the opinion there expressed, that, though these diseases may simulate the symptoms of narcotic poisoning, yet "the history of the case, the odour of the breath, and the subsequent examination of the body after death, will generally clear up the difficulties which may at first present themselves." (161.) Even with these aids, we fear the medical jurist can effect little in charges of poisoning with the narcotics, when there is no moral proof of a poison having been administered.

The second question in general poisoning which they consider is, Whether the alterations of tissue produced by poison may also be produced by natural causes. Their remarks on this question are limited to erosion of the stomach, which is now well known to be the effect of natural agents as well as of corrosive poisons. They first relate the circumstances under which *Spontaneous Erosion* occurs, then its external characters, and finally, its diagnosis from corrosive dissolution of the stomach; and they subjoin a long and valuable note, with observations on the celebrated trial of Charles Angus, for the murder of his house-keeper, Miss Burns. The investigation of this curious subject, so far as it goes, is sound and instructive, but betrays a lack of information, which we were not prepared to expect from the learning of Dr Paris. He has confined his attention entirely to what has been written regarding it by English authors, who are neither the most prolific, nor the most precise; and, in consequence, he has adopted unreservedly the views of Mr John Hunter and Mr Allan Burns respecting the causes of this curious pathological phenomenon. According to these views, spontaneous erosion is a process carried on after death by the solvent powers of the gastric juice. But Dr Paris cannot be ignorant, that in France and Germany, where it has been much oftener seen, and much more carefully investigated, a very different opinion is entertained of its nature,—that it is believed to be the effect of a peculiar disease, and to take place during life. Those who wish to examine what foreign authors have said concerning it, will find an essay on it by Jaegar of Stuttgard, in Hufeland's Journal for 1812-13; * another in a pathological work by Cruveilhier of Paris; † and a very valuable thesis, written expressly on the subject, by one of Professor Chaussier's pupils under the eye of his teacher. ‡ Many good cases are also to be found throughout the foreign periodicals; and the English reader will find an excellent summary of the whole, with some new cases, in a paper by Dr J. Gairdner of this city in the recently published volume of the Edinburgh Medico-Chirurgical Society. The origin of Spontaneous Erosions and Perforations is still involved in doubt and mystery. On the one hand, the French and German writers have related cases in which the nature and progress of the symptoms can scarcely leave a doubt that the perforation found after death must have taken place during life, as the result of a peculiar morbid process. § On the other hand, Mr Allan Burns has shown experimentally, that dissolution of the stomach by its own juices, may be carried on long after death; || and

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* Analyzed in Lond. Med. Rep. vol. x.

† Médecine-pratique eclairée par l'Anatomie et Physiologie Pathologique.

‡ Considérations sur les Perforations de l'Estomac, par G. Laisné. 1819.

§ See particularly the Dissertation of Laisné.

|| See this Journal, vol. vi.

the numberless instances on record, in which vast erosions have been found in the stomachs of those who had not a single symptom of an affection of that organ, and decidedly died of a different disorder, prove unequivocally that the process cannot always have been a vital one. The information yet possessed concerning it does not enable us to advance any thing but pure hypothesis for reconciling these discrepancies.

In commenting on the instructive case of Miss Burns, our authors have dropped an opinion, which, however reluctant we are to approach the unhappy controversy which followed the trial, we nevertheless cannot pass without notice. " Whether, " say they, " the holes in the stomach were the effects of corrosive poison, or of that solvent action after death, which we have just endeavoured to explain, must remain a matter of doubt; for the erosion in this case was so considerable, and the inflammation so slight, that it is impossible to assert that they both depended on the same cause." (p. 177.) Leaving out of view the *non-sequitur* of this opinion, we shall beg to ask Dr Paris, what poison would have produced so large a perforation in so short a time? only, we apprehend a strong mineral acid, or oxalic acid, or a mineral alkali, or, *perhaps*, corrosive sublimate. But if either of these substances had produced them, (laying aside the impossibility of the lady swallowing the requisite quantity without discovering that she had taken some dreadfully deleterious drug, and even laying aside, too, the incompatibility of the actual appearances in the stomach and elsewhere with those resulting from the substances mentioned), we assert decidedly, and without fear of being contradicted by any impartial toxicologist, that the poison must have been easily discoverable by chemical analysis. On the other hand, the case of Miss Burns, in the symptoms, in its progress, in the pathological appearances, and in *the collateral circumstances also*, is so identical with several recorded instances of spontaneous erosion, particularly with some of those narrated by Chaussier in Laisné's Dissertation, that we cannot hesitate to class it along with them. Dr Paris's opinion might have been a very proper and prudent one in the year 1808, when the controversy took place. But after so much light has been thrown on the subject since, it is a dangerous one to be held in a work whose authority may be referred to and adopted by witnesses in future and similar cases.

The *third* question, Whether rapid putrefaction affords any presumption in favour of poisoning, scarcely admits of a precise answer while stated in such general terms. Our authors remark, that " as far as their observations enable them to deduce any conclusion, certain vegetable poisons appear to accelerate such a change; for very shortly after death, the body, under such circumstances, will frequently swell, become highly offensive, assume a black appearance, and exhibit gangrenous (*gangrenous-like**) spots on its surface. No such appearances, however, it is said, usually follow as the *specific* consequence of the fatal operation of mineral poisons." (p. 182.) These remarks are vague and unsatisfactory. The authors are quite correct indeed in stating rapid putrefaction to be an occasional consequence of death by poisoning with certain vegetable substances: The narcotics do at times produce this effect. But it is very far from being invariable; and the circumstances under which it may be looked for are still unknown. It is evidently connected, however, not with any intrinsic property in the poisons themselves, but with the kind of death they occasion; and granting that the narcotic vegetables did invariably cause rapid decay, this fact could constitute no argument in favour of poisoning, since various natural diseases kill in the same manner, and are followed by the same phenomena. Very opposite opinions have been entertained with respect to the power of the mineral poisons in affecting the progress of putrefaction. Not long ago they were generally thought to promote it; and the authors have quoted two remarkable trials for poisoning, one so late as 1808, at which an argument was drawn against the accusation from the slow decay of the body. More recently, however, some continental writers have advocated the very opposite opinion. They have maintained that arsenic in particular is a powerful antiseptic, even in the small proportion in which it is used for the purpose of poisoning. This idea derived a specious stability, from two singular events which occurred not long ago in Germany. On one occasion, the bodies of five persons, poisoned with arsenic by a lady of rank, were disinterred two years and a half after burial, and found converted into a sort of mummy.† On the other occasion, the bodies of three persons poisoned in the same way were found quite fresh and entire between two years and six months after interment.‡ The subject has since been investigated by Guyton-Morveau, Klank, Welper and Jaegar; and the result of their united researches seems to be, that arsenic is an antiseptic, when *applied directly* to any part of the body in sufficient quantity; but that, when simply swallowed, it has no power whatever either in retarding or in favouring putrefaction. These speculations, though applied by a hasty and unjustifiable generalization to poisoning by the whole class of irritants, rest entirely on the supposed properties of arsenic. No cases or experiments have been ever brought forward to show that any other irritating poison possesses similar properties.

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* We could point out numberless instances of this loose and inaccurate phraseology in their work. It is this vague phraseology which has been the occasion of many mistakes by medical witnesses; and we cannot help expressing our surprise that it should have found a supporter in so modern and so classical an author.

† Hufeland's Journal, XVI.

‡ Mem. Med. & Phys. Soc. Erlangen. See also Harles de usu Arsenici, a work which contains the best account yet given of this poison, and, in particular, a full and impartial investigation of the controversy alluded to above.

We come next to the *fourth* question, Whether the fact of poison not being discoverable in the alimentary canal is a presumption against a charge of poisoning?—a question which, however self-evident it may appear to the experienced toxicologist, has been most properly investigated by our authors; since it is consistent with our frequent observation, that very mistaken ideas prevail respecting it throughout the greater and less instructed part of the profession. Dr Paris justly observes, that, although the tests for the chief poisons have been rendered exceedingly delicate, and are by no means difficult of application, yet the failure of them to demonstrate the presence of a suspected poison in the alimentary canal is no proof that it was not actually given; for it may have been all discharged by vomiting and purging, or it may have been all absorbed, or it may have been decomposed. He is not very explicit regarding the kind of decomposition here alluded to. But it should be understood, that the only kind, which, of itself, can render poisons not discoverable, is the decomposition effected in vegetable substances by the digestive processes. The most common cause of failure is the discharge of the poison by vomiting and purging: Of this an admirable illustration is quoted by Dr Paris from our Number for April 1822. A less frequent cause is absorption: The activity with which this function is carried on in certain circumstances, appears to be the chief reason why several analysts of repute have failed to detect the hydrocyanic acid in animals killed by it. * Of the power of digestion in destroying the characters of poisons, so far as to render them undistinguishable by the eye of the druggist or the operations of the chemist, we have seen a very interesting example in a foreign Journal, the reference to which, however, has been unfortunately lost. It was the case of a person who died some hours after swallowing a considerable quantity of solid opium, and in whose body none of the drug could be detected, either by its sensible properties, or the most refined chemical analysis.* Although we agree, on the whole, with the opinions expressed by Dr Paris and Mr Fonblanque on this question, yet they must be received with some qualification. In particular circumstances the non-detection of the poison *may* be a sufficient argument against a charge of poisoning; for these circumstances may be such as altogether to preclude the possibility of the poison being either ejected, or absorbed, or decomposed. This qualification they ought to have noticed, considering the numberless persons who will interpret their opinions according to the strict letter.

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* For some observations on the effect of absorption, see Vol. xix, p. 196, of this Journal.

Before taking leave of the topic before us, we beg to warn Dr Paris of an oversight he has committed in mentioning the experiments of Dr Bostock, on the possibility of poison not being found after it has decidedly been given. "To Dr Bostock the judicial physician is under many obligations; but there is no discovery for which he is more deeply indebted to him than for that which has resulted from his satisfactory experiments † in elucidation of the present question. He has shown, in the instance of corrosive sublimate, that an animal may be suddenly killed by receiving a metallic poison into the stomach, and yet that the most delicate chemical re-agents may not be able to detect any portion of such poison, after death, in the contents of that viscus." (p. 187.) We are very far from harbouring the remotest desire to detract from the merits of Dr Bostock: But we must be forgiven by him for hinting, that the experiments alluded to, though quite conclusive so far as concerned the particular object for which they were made (namely, to show that the failure of *his own* experiments on the contents of the stomach of Miss Burns was no presumptive evidence against poisoning), do not at all apply, as Dr Paris thinks, to the general question, and do not demand the high encomiums here lavished on them. If Dr Paris will review these experiments again, he will find, that, in analyzing the alimentary matters taken from animals which had been poisoned by corrosive sublimate, Dr Bostock tried only the fluid part, and that not till thirty hours after death. ‡ Now, under these circumstances, the quantity of poison being minute, no mercury could remain in solution; it must have been all thrown down as an insoluble protochloride in union with animal matter; and therefore should have been sought for, not in the *fluid* part of the contents of the stomach, but in the *solid* part, or in the coats of the stomach itself.

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* A similar case, though not so strong, is described in Journ. Univ. des Sc. Méd. vol. xix. 340.

† See Trial of Charles Angus, or Vol. v. of this Journal.

‡ Edinburgh Medical and Surgical Journal, Vol. xiv.

The last question in general poisoning, noticed by Dr Paris and M. Fonblanque, relates to the information which may be got by administering the suspected contents of the stomach to dogs and other animals. This question they have not investigated skilfully. In the precise shape in which they have stated it, a very ready answer may be given; for no doubt can be entertained, that the contents of the stomach, taken from persons who have died a natural death, occasionally act as poisons on the lower animals. But laying this fact altogether aside, experiments with animals are evidently unnecessary; since, if the suspected matters contain poison enough to kill them, its presence must be demonstrable, beyond all risk of uncertainty, by chemical analysis. But the question must be answered differently, when viewed in relation to other suspected substances—for example, articles of food or drink. The effect of these on animals may furnish perfectly valid evidence; and this it is essential to understand, because, on trials for poisoning, important evidence of the kind may be supplied *accidentally*, in consequence of cats or dogs happening to devour the remains of a poisoned meal. Another still more important object to be attained by experiments on animals, is the determination of disputed points in the physiology or pathology of poisoning. We mention these particulars, in order to put the medical witness on his guard; for, on several occasions, our courts of law have attempted to reject every kind of evidence from experiments on animals, on the ground that they do not furnish trustworthy proof under any circumstances; and it is certainly his duty to resist and prevent, if possible, such erroneous notions. The late researches of Orfila and a host of other experimenters have proved to demonstration, that on several domestic animals, particularly the dog and cat, poisons of every kind (or at least with only one or two exceptions) cause precisely the same effects as on man. The further prosecution of these researches, we may with reason anticipate, will likewise show, that the same rule holds in regard to other animals. It is true that many singular statements have been published regarding the diversity in the effects of poisons on different tribes of animals. These statements, however, are often contradictory, and seldom founded but on vague observation. On the other hand, it is well ascertained, that many of the energetic poisons act nearly in the same manner on all animals whatsoever, from the highest to the lowest in the scale of perfection. Both reasoning and experiment render it highly probable, indeed, that some orders of animals, for example the Ruminantia, are less sensible to the action of poisons than man and other animals. But, so far as may be judged from the only accurate inquiries on the subject, their effects differ in degree, not in kind. Some exceptions, without doubt, do exist to this rule; but they will be found much fewer than is generally imagined.

We have selected as the subject of the foregoing remarks the most interesting and most practical part of Dr Paris and Mr Fonblanque's treatise on General Poisoning. Viewing it collectively, we can find in it neither much to praise nor much to blame. It is not by any means comprehensive: The authors, in composing it, do not appear to have kept in view, as they should have done, the whole range of toxicology. Although the general principles laid down are on the whole sound, yet, in unfolding them, they sometimes fall into error, often show a want of precision both of thought and language, and are apt to exhaust their whole comments on one solitary practical illustration, leaving others of greater consequence nearly or altogether unnoticed. On many occasions, too, they betray a deficiency of information we were not prepared to expect in a work professedly so learned; and, above all, they harass the reader and the consulting student by the imperfections of their arrangement. The primary arrangement of the book, as we formerly suggested, is radically loose and irregular; and these defects appear to us to extend throughout every subdivision and ramification of it. But, notwithstanding these drawbacks, there is an expression of interest on the face of the whole treatise we have been analyzing, which cannot fail to rivet the attention, particularly of the general student. Few works of the kind will leave a more favourable impression after a first reading.

As an object of comparison, we shall now take a cursory view of that part of Professor Orfila's Lectures which treats of general poisoning. In doing so, we shall merely present an abstract of the treatise, as our opinion has already been fully expressed on most of the doctrinal points discussed in it.

"Having traced the characters and effects of each poison in succession," says he, "we must now proceed to regard the subject of poisoning in a general point of view, and endeavour to settle the two following questions; 1. How are we to recognise a case of poisoning? 2. And, how are we to determine what poison has been administered?" p. 335.

The circumstances which must be taken into account, when we attempt to recognise a case of poisoning—viewing the word in its general sense—he considers to be the following. 1. The symptoms; 2. The alterations of tissue; 3. The diseases imitating the effects of poison; 4. Experiments on animals. The two first of these heads are discussed too briefly; and on the inference to be drawn from the symptoms he has expressed, as we have already said, too unqualified an opinion. The imitative diseases are cholera, spontaneous erosion, acute gastritis, the iliac passion, strangulated hernia, volvulus and other intestinal obstructions, peritonitis, hæmatemesis, melæna. Under the head of Experiments on Animals, he first notices the best mode of administering poison to them, namely, by opening the œsophagus, introducing the poison through this aperture, and tying the canal farther down; and then mentions the inferences to be drawn from the general effects produced. Sometimes the animal perishes in a few hours under the symptoms which are known to characterize the poison under suspicion; or, *secondly*, no symptom whatever appears for forty-eight hours after the requisite operation; or, *thirdly*, it is affected with nausea for four-and-twenty hours, with efforts to vomit, and other symptoms of poisoning, and recovers in a day or two; or, *lastly*, it experiences some of these symptoms three or four days after the operation. For the inferences to be drawn from each of these supposed effects, we must refer to the work itself, as they are incapable of abridgment.

The second problem, namely, to discover what poison has been given, he has treated at great length, and with consummate ability. The means to be employed for this purpose are derived, 1*st*, from physical and chemical characters; 2*d*, from the peculiar symptoms; 3*d*, from the morbid appearances; but the two latter means are merely accessory to the first. The application of the physical characters of poisons, for the purpose of detecting their nature, must be necessarily limited; and it is therefore to their chemical distinctions that Professor Orfila has directed all his attention. He commences the inquiry with some instructions to the medical jurist with respect to the legal formalities, the preparations he must make, the mode of removing the contents of the stomach and intestines, and the mode of preserving them. He then observes, that the test of chemical analysis can be applied only to the mineral, and to a few of the vegetable poisons. The order of analysis he recommends for these is the following.

In the *first* place, the poison may be *pure*. Of the pure poisons some are solid, others fluid, others gaseous. Of the solid poisons, some are mineral, others vegetable in their nature. Of the former, some have a white or grayish colour, others are yellow, green, blue, red or black. Of those which are white or gray, some are soluble, others insoluble, in water. Of the solutions of the white solid poisons, some are precipitated by the sulphuretted hydrosulphate of potass (dissolved liver of sulphur); others remain unchanged. Of those precipitable in this way, some are also precipitated by pure potass, others are not affected by it. Of those which do precipitate with potass, some yield a white or whitish precipitate, others precipitates of different colours; and of the former, some precipitate with hydrochloric acid, others do not. Of the solutions of the white solids which do not precipitate with the sulphuretted hydrosulphate of potass, some turn the syrup of violets green, others do not affect it. Of the former, some precipitate with subcarbonate of potass, others do not; and of those which do not, some precipitate with the hydrochlorate of platinum. Of those which do not affect the syrup of violets, some precipitate with sulphuretted hydrogen, others do not; and of the latter some yield a precipitate, and others none with the subcarbonate of ammonia. The insoluble white poisons are few in number, and easily distinguished from one another, each by its own test. The coloured solid mineral poisons are some of them soluble, others insoluble, in water. Of those which are soluble, some are not affected by the sulphuretted hydrosulphate of potass, others yield variously-coloured precipitates. The insoluble coloured solids are first separated into groups, by their peculiar tint of colour; and after this has been done, it is easy to recognise each by its own test. By analogous processes of reduction, the analyst is also enabled to distinguish the solid vegetable, as well as the fluid and gaseous poisons. It is unnecessary, however, for us to travel through these processes; we have given the foregoing abstract of the examination of a solid mineral poison, merely as a specimen of the extensive chemical knowledge which, in such inquiries, the medical jurist may be expected to possess.

But, *secondly*, the poison may be mixed with tea, coffee, or other articles of food and drink, or with the matters of vomiting, or with the contents of the stomach and intestines. The process recommended by Orfila for examining such mixtures is as follows. Any solid matter which can be separated, must be subjected to the researches already detailed. This being done, the fluid part, if coloured, is to have its colour destroyed by chlorine, unless we find that it contains nitrate of silver, or an antimonial salt, which are easily known. The colourless fluid is then to be gently evaporated in a retort fitted with a receiver. The distilled fluid is to be subjected to the process laid down for the liquid poisons; and what remains in the retort having been allowed to cool, any insoluble matter which is thrown down is to be treated as already mentioned; and then, finally, the remaining fluid is to be subjected to its proper process. If all these means fail to discover the true nature of the poison, another process still remains to be tried. The mixed liquid is to be evaporated to dryness, and a part of the residue calcined with a mixture of charcoal and subcarbonate of potash in a glass-tube. If arsenic or mercury be present, they will sublime, and condense on the inside of the tube, the former in a brilliant scaly crust, the latter in white globules. If any other metallic poison existed in the suspected mixture, it will be found reduced in the residue of the calcination; and each metal may be recognised without difficulty by its proper tests.

The whole of this most elaborate dissertation on the analysis of suspected substances, when the actual poison is unknown, well deserves the careful consideration of every student of medical jurisprudence. It is the only good attempt which has yet been made to solve the general problem in question; and we have no doubt, the rules and order of investigation recommended will be found highly useful in practice.

Pursuant to the plan we proposed to follow in examining the work of Dr Paris and Mr Fonblanque, we should now attempt a critical analysis of their dissertations on some of the principal poisons. But as this article has already extended itself farther than was our original intention, we shall be content with selecting one only, to wit, Arsenic, the commonest, and therefore the most important of them all.

They commence their treatise on arsenic, by a succinct account of its history; and then describe the physical and chemical properties of the white oxide and the metal; next the symptoms it produces; next the different modes of poisoning with it; next its physiological action; next the organic derangements it produces; and, finally, the tests of its presence in its pure and mixed state.

In noticing the chemical properties of the metal, several little inaccuracies have crept into their description. Its subliming point is said to be 540° Fah.; whereas all the best modern authorities state it to be only 356° [*]; and no chemist, so far as we know, has fixed it higher than 388°. [†] Their remarks on its fusibility would also lead the inexperienced reader to infer, that it has not, and cannot be melted. Now, Thenard has stated, that, with the aid of pressure, it may be melted at a temperature nearly the same as the fusing point of tellurium, and somewhat less than that of lead. [‡] "Arsenic," say they, "is extremely susceptible of oxidation, and by mere exposure to the air, shortly loses its metallic lustre." This is not invariable; for reasons not very well understood, it sometimes does not oxidate.[*] These are trivial errors, it is true; but, considering the purpose of such a work, are not altogether excusable, particularly as many readers would at once, and with some show of reason, infer, that a similar inaccuracy must pervade its more important parts. The account which follows, of the physical and chemical properties of the oxide, is correct and appropriate; and contains some important practical remarks on the production of the alliaceous smell by its reduced vapour.

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[*] Thomson, Thenard, Orfila, &c.

[†] Bergmann.

[‡] Traité de Chimie, i. 202, 325.

" In the state of vapour it is quite inodorous, although the contrary is positively asserted in several chemical works of high authority. The fact is, that the alliaceous, or garlic-like smell, is wholly confined to metallic arsenic in a state of vapour; and whenever the arsenious acid seems to yield such an odour, we may very confidently conclude that its decomposition has taken place, and that it has been reduced to its metallic state. Such a reduction will generally happen when it is projected upon ignited charcoal, or when heated in contact with those metallic bodies which readily unite with oxygen, such as antimony, zinc, &c. It is stated by Orfila and other writers, that if it be projected upon heated copper, the alliaceous odour is evolved. This assertion is undoubtedly true, but the fact requires to be explained with more precison, or we may fall into an important error respecting it. The phenomenon takes place only when the copper is in a state of ignition, at which temperature its affinity for oxigen enables it to reduce the arsenious acid, and consequently to develop the metallic odour. We have ascertained by repeated experiments, that if a few grains of arsenious acid be heated on a plate of copper by a spirit-lamp or blow-pipe, no odour is perceptible; for in this case, the whole of the acid will be dissipated before the copper can acquire a temperature sufficiently exalted to oxidize and reduce it. If the arsenious acid be heated on a plate of zinc, the smell will not be evolved until the latter metal is in a state of fusion. If, instead of the foregoing surfaces, we employ gold, silver, or platinum, no alliaceous smell whatever is produced at any temperature, provided every source of fallacy be carefully avoided. But it deserves particular notice, that the author has found the flame of the spirit-lamp to be in itself capable of decomposing the arsenious acid, in consequence, it is presumed, of the operation of its hydrogen, (carburetted hydrogen)." p. 213.

The symptoms of poisoning with the arsenious acid, or oxide of arsenic, are related in considerable detail, and with great accuracy; but the authors do not seem to have seized the leading distinctions between the different cases, according to their duration.

" Poisoning with arsenic may be considered as admitting of three degrees of intensity. 1*st*, Where the case, although attended with dangerous symptoms, does not terminate fatally; 2*d*, Where death does not follow until after a lapse of twenty-four hours; *3d*, Where death takes place within twenty-four hours after the exhibition of the poison."

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[*] Berzelius, Ann. de Ch. & De Phys. xi. 240.

This is the arrangement first proposed, and followed by Hahnemann; but it does not appear to be founded on any very obvious difference in the symptoms. A much more natural division would be the following. *1st*, Where the person dies betwixt twenty-four hours and two or three days; *2dly*, Where he expires in five, six, or ten hours, or at farthest within the first day; *3dly*, Where life is prolonged six, eight, or ten days, or is saved altogether, but after a tedious illness; and, *4thly*, Where the person recovers after a single day's illness. In the first variety of cases, the symptoms are chiefly those of violent irritation of the stomach and intestines, combined with excessive prostration, occasionally with signs of irritation of the other mucous surfaces, but seldom with nervous symptoms. In the second variety of cases, the person expires before the signs of local inflammation are fully developed, in consequence, it is commonly supposed, of some impression on the general system. In such cases, the symptoms are very peculiar, and totally different from those of ordinary occurrence. They happen chiefly when the poison has been given in solution, or in very large quantity, or in fragments, or when it has been applied to external bleeding surfaces. The *rationale* of the operation of the first and last of these circumstances is obvious; that of the other two is less obvious. For examples of this kind of action, we may refer the reader to the cases noted below. * In the third description of cases, viz. when the person survives six, eight, or ten days, or recovers altogether, there are added to the symptoms of irritation in the stomach and intestines, signs of a similar affection of most of the other mucous surfaces—the mouth, nose, conjunctivæ, trachea, lungs, bladder, vagina; and likewise, very generally, symptoms of disorder in the brain, to wit, delirium, or coma, or partial palsy, or epilepsy, or hysteria, or several of these together. The most remarkable recorded cases of this description are contained in our Journal for October 1819, and April 1822,—in the second volume of the Medico-Chirurgical Transactions,—and in Mr Marshall's Essay on Arsenic, published on occasion of the trial of Eliza Fenning. The fourth class of cases includes those who, after a few paroxysms of vomiting, and slight signs of irritation in the alimentary canal, recover in a day or two, without any lasting or permanent inconvenience. The distinctions thus laid down are sufficiently marked, and would lead to several important practical inferences. But these we cannot enlarge on in the present place.

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* Brodie's Experiments, Phil. Trans. 1812.—Jaegar's Expts. Ed. Med. Surg. Journ. vii.—Sprœgel's Expts. in Haller's Disp. Med. Pract. vi.
Laborde—Journal de Médecine, lxx.—Crowfoot, Med. Phys. Journ. xxxiv.—Renault, sur les contreposions de l'Arsenic, p. 100—Morgagni, Epist. Anal. lix. 5.

To the description of the symptoms, succeeds an account of the different modes in which poisoning may be effected with arsenic; and the authors give instances of its acting, not only when swallowed, but likewise when introduced into the rectum, into the vagina, or when applied upon a wound or sore. They might have also added, that the mere application of it to the unbroken skin may cause symptoms of poisoning. At least, a case to this effect has been related by Desgranges, in the 6th volume of the *Recueil Périodique de la Soc. de Médecine;* another very remarkable one is contained in the 22d volume of the *Acta Germanica* (1730); and Renault obtained similar results by experiments on animals.

The next head embraces an account of its physiological action. Upon this subject their remarks are very brief. They show that it acts through the medium of absorption; that, in whatever way it enters the system, it excites inflammation of the stomach, and frequently of the rectum also; but that the derangement of structure in these parts is seldom the cause of death. They have made no attempt to determine in what way death is actually occasioned.

Some very judicious observations then follow on the organic lesions found in the bodies of those poisoned by arsenic. These are signs of inflammation in the stomach, namely, redness, softening, or abrasion of its villous coat, and ulceration, sometimes penetrating the whole coats; inflammation in the duodenum, and even of the rest of the intestines; also inflammation of the pharynx and œsophagus. No appearance of note is found in other parts of the body, except fluidity of the blood. They have omitted to mention, that both the trachea and urethra are sometimes found inflamed, * and occasionally also the pleura and lungs; † that the penis is sometimes distended and black, and the whole body frequently very livid. These appearances, indeed, are neither constant nor characteristic signs of death from poisoning with arsenic; but nevertheless deserve notice in an enumeration of its effects. Softening of the villous coat, the authors represent to be almost always present: this must be an exaggerated statement; we certainly have found the villous coat entire and strong in animals; and the same account is given in many published cases. They have added, that ulceration of the coats of the stomach sometimes pierces them even to complete perforation. This is a common idea among medical jurists; but we are not acquainted with a single fact which authorizes it. Even ulceration is very uncommon, obviously because the patient in general dies too soon for this process to be completely established. The blood, they observe, retains its fluidity; an observation which they rest on the experience of Brodie, Ruysch, and Jaegar. But fluidity of the blood is by no means a constant phenomenon; Wepfer, in his experiments, found polypous concretions in the heart; so did Dr Campbell, as stated in his Inaugural Dissertation; and we have likewise had occasion to observe it in the same state.

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* Jaegar's Expts. Ed. Med. and Surg. Journ.

† Dr Campbell's Inaug. Diss. Edinb. 1813, and Sprœgel's Experiments in Haller's Disp. Med. Prac.

By far the most precise and accurate part of the treatise on arsenic, is that appropriated to the consideration of its tests. Tests may be required under two circumstances; 1. When the poison is in a state of purity; 2. When it is mixed with various articles of food, or with the contents of the stomach, or matters of vomiting.

1. When it is in a state of purity, its tests are—reduction to the metallic state, and sublimation—the formation of an argentine alloy with copper—the formation of an arseniate when heated with any nitrate—the production of a yellow precipitate in its solution, when mingled with the nitrate of silver, an alkali being previously added to the arsenical solution, or with the ammoniacal nitrate of silver, without the alkali—the production of a grass-green (sometimes apple-green) precipitate, with the ammoniacal sulphate of copper—of a golden yellow precipitate with the sulphuretted hydrogen, or the alkaline hydrosulphates —and of a white precipitate, with lime-water. These tests have now been brought to an extreme degree of delicacy and perfection. The test of reduction is recommended to be performed in a glass tube eight inches long, coated with clay, and placed for ten minutes in a chaffing-dish of coals. This is a very clumsy process; so that we do not wonder Dr Paris has been obliged to confess he could not detect less than three quarters of a grain by it. A much better plan is that recently recommended by Mr Phillips or Dr Trail, * which consists in using a tube of much less size, and exposing it naked for half a minute to the heat of a spirit-lamp, or the flame of a common blowpipe lamp. In this way, Dr Traill has detected a tenth part of a grain of the oxide. We may add, that we have been long in the habit of employing the same process, and have several

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* Annals of Philosophy, Jan. & Feb. 1824. See the Domestic Intelligence of this Number.

times succeeded in detecting by it so small a quantity as a sixteenth part of a grain. For effecting the reduction, the black flux, it should be noticed, is much preferable to a mixture of charcoal and potass, as proposed by some chemists; for when such a mixture is used, it is very difficult to prevent the charcoal powder from being carried along with the aqueous vapour, and so forming a black crust on the inside of the tube.

The second test, namely, the whitening of copper by the formation of an alloy of this metal with arsenic, is so very coarse and clumsy, that, leaving altogether out of view its fallacious nature, we cannot help being surprised that chemists and medical jurists have not long ago discarded it entirely. In all circumstances in which it admits of trial, the securer and more delicate test of reduction and sublimation may be also employed; and such being the case, the other becomes totally useless.

The formation of an arseniate by fusion with nitrate of potass, is an excellent test of arsenic, both in its pure and impure state. If the suspected substance be fused with nitre, and the product, after solution, treated with the nitrate of silver, a brick-red precipitate is thrown down, if any arsenic was present.

The next test is the nitrate of silver, which, with the previous addition of an alkali to the suspected solution, causes a yellow precipitate, if arsenic be present. The most approved method of employing the nitrate of silver, is to use the fused nitrate in solution, and, instead of adding an alkali to the suspected fluid, to drop ammonia into the test, till the brown precipitate at first thrown down is almost, *but not altogether*, re-dissolved. In this way, the test possesses the advantage of not causing a precipitate in solutions of the phosphates; a fallacy which constituted the principal objection to the former mode of using it.

The sulphate of copper is also best applied as a test for arsenic, by adding ammonia to its solution, till the blue precipitate at first thrown down is *almost* all dissolved again. It causes an apple-green or grass-green precipitate. Another very useful mode of using the sulphate of copper has been lately proposed by Mr Phillips. * He first drops potass into the solution of the sulphate, and thus causes a light blue precipitate. On then adding the suspected fluid, it will change the colour of the precipitate to an apple-green or grass-green, if it contained arsenic.

Sulphuretted hydrogen, either added in solution, or transmitted through the arsenical fluid in its gaseous state, causes a rich golden precipitate. This is at once the most delicate and the most unequivocal of all the watery tests. A few other metallic solutions, such as those of antimony and tin, are similarly affected by it; but the distinction between them and arsenic is easily established, by using at the same time the foregoing re-agents.

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* Annals of Philosophy, Jan. 1824. See also the Domestic Intelligence of our present Number.

Lastly, lime water causes a white precipitate with the oxide of arsenic, which precipitate is soluble in an excess of the oxide.

The account we have thus given of the tests, as laid down by our authors, for the poison in its pure state, is wound up with the following judicious and important remarks on their relative and conjunct value.

" In taking an impartial review of all the evidence which the investigation of this subject can furnish, it must appear to the most fastidious, that the silver and copper tests above described are capable, under proper management and precaution, of furnishing striking and infallible indications; and that, in most cases, they will be equally conclusive, and in some, even more satisfactory in their results than the metallic reproduction, on which so much stress has been laid; and for this obvious reason, that unless the quantity of metal be considerable, its metallic splendour and appearance are often very ambiguous and questionable." [We apprehend, however, that no dexterous and careful person should ever encounter ambiguity in the matter.†] " But the question at issue may be easily disposed of to the satisfaction of all parties; for, let it be remembered, that the application of chemical re-agents to solutions suspected to contain arsenic, so far from throwing any obstacle in the way of the metallic reproduction of that substance, are the very steps which should be adopted as preparatory to the *experimentum crucis*. It is only necessary to collect the precipitates, and to decompose them in the manner already described; and this confirmation of our results should never be neglected." p. 251.

2. But, *secondly*, the oxide of arsenic may be mingled with various animal and vegetable matters, which obscure the action of the re-agents we have mentioned. In that case, the authors recommend that a solution of the ammoniacal nitrate of silver be added to the filtered fluid, as long as any precipitation takes place; and that the precipitate be then exposed to the test of reduction. The sulphuretted hydrogen is preferable to the silver test in this process. Sometimes it may be sufficient to

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† The outer surface of the crust, that, namely, next to the tube, has a brilliant appearance like polished steel; the inner surface is crystalline, either to the naked eye, or at all events, under a magnifier of four or five powers. If any doubt should still remain, let the tube be broken, and its encrusted fragments thrown into a diluted solution of the ammoniacal sulphate of copper; in a few hours the metal will either become grass-green, or if very minute in quantity, will be disolved, and a grass-green crust formed on the surface of the fluid. (R.)

decolorize the suspected fluid with chlorine, and then apply the liquid tests in the usual way. This process was first proposed by Orfila, and is useful in many cases, though more generally inapplicable. If the stuff with which the poison is mingled be too viscid to pass the filter, it is to be evaporated to dryness with a moderate heat, and then exposed to a subliming heat in a proper vessel. The sublimate, being either the oxide or the metal, according to circumstances, is to be recognised by its properties already enumerated. Sometimes it may be requisite to analyze the coats of the stomach itself. The process proposed for this purpose is too long to be noticed here. It was originally recommended by Rose, and has been adopted both by Orfila and Dr Paris. But it will be very seldom necessary.

The subject of arsenic is concluded by some observations on the arsenic acid, the arseniates, and the sulphurets. Among the preparations of this substance, met with in commerce or the trades, and therefore liable to become the cause of poisoning, three compounds have been omitted, two of which are of considerable importance, since they are in common use as pigments, viz. the fly-powder, *(fliegenstein, poudre à mouches)*, or the black oxide; king's yellow, an impure sulphuret; and mineral green, the arsenite of copper. The two last of these are not mentioned at all; and the first is barely touched upon when they speak of the metal becoming tarnished in the air. All these have frequently been the cause of dangerous mistakes, and the two last have been used in this country for criminal poisoning.

It was our intention to extend this analysis to a few other poisons; but our observations have already been prolonged too far. We apprehend that the foregoing strictures on the disquisition of Dr Paris and Mr Fonblanque will unfold sufficiently to the reader the style in which they have treated the subject of the Individual Poisons. They have certainly been peculiarly happy in selecting and pourtraying the most striking and attractive points in their history, mode of action, and medicolegal relations, and cannot fail to interest deeply the general reader. But we doubt whether this part of the work is exactly calculated for the purposes of pure science. Several subjects of inquiry will occur in practice, for which medical jurists will search the work in vain; and it is evident, that, on many of the points which are discussed, its authority cannot be altogether relied on; for it contains some inaccuracies, many omissions, and not a few proofs of defective information. The justice of these criticisms might be put in a still clearer light, if we were to scrutinize the rest of the Toxicology as minutely as we have done the chapter on Arsenic; for that chapter is decidedly the most unexceptionable in the whole book. If, for example, we were to examine what they have said on poisoning by Copper, Lead, the Fixed Alkalis, Ammonia, Baryta, Zinc, and especially Nux Vomica, it would not be difficult to show how a much better treatise might have been written on these subjects.

It must be confessed, however, that the ideas we entertain of a full and sufficient treatise on Toxicology, as a branch of Medical Jurisprudence, cannot be easily reconciled with the succinctness which is so desirable in a systematic work, and for the preservation of which Dr Paris has probably been induced to curtail this department of his subject. But we may remind him, that Toxicology scarcely forms a branch of any other medical science, and cannot be fully discussed any where with so much propriety as in a treatise on Legal Medicine. Had the authors devoted to it all the space which they have wasted on their Appendix, the improvement, we are well assured, would have been generally acceptable.

V.

Commentarium de Urethræ Corporis Glandisque Structura VIto Idus Decembris ann. M. DCC. X. detecta, Ab I. R. Scientiarum Literarum Artiumque Instituto approbatum, Alexandri Moreschii Equitis Coronae Ferreae in Ticinensi primum, tum Bononiensi Archigymnasio Anatomes Professoris. Accedunt de Vasorum Splenicorum in Animalibus Constitutione, nec non de Utero Gravido Epitomae. Cum Tabulis Aeneis. Mediolani, ex typographia Joannis Pirottae. 1817.

Exactly two years ago, we took occasion, in our account of the researches of Sir Everard Home on the structure, and those of Professors Tiedemann and Gmelin on the functions of the spleen, to speak of what has been termed by Dupuytren, Richerand, and other French anatomists, the *Erectile Tissue.* We also observed that the nature and intimate arrangement of this tissue had been studied chiefly in the cavernous body of the penis, and the peculiar structure surrounding the male urethra, to which anatomists had applied, in regard rather to its appearance than intimate characters, the name of *spongy body.* Of the anatomists who have distinguished themselves by personal researches into the characters of this tissue, Alexander Moreschi, Professor of Anatomy, first in Pavia, and afterwards in

Bologna, may be said to hold a conspicuous place; and the work now before us, which contains his original researches, may be regarded as the most authentic and satisfactory account of the subject, which has hitherto appeared before the public.

The author informs us, that in the beginning of December 1810, when tracing the nervous twigs of the crural plexus, as he had injected most of the minute vessels to their smallest extremities, he thought of rendering the preparation more complete by injecting those of the penis also; and with this view, he first distended the cellular or cavernous body with green coloured matter, and afterwards that of the urethra with red coloured injection. The result of this experiment was, that the matter injected, instead of diffusing itself extensively through the cellular structure of the glans, as had always before happened, formed on its whole surface an intricate net-work of very minute vessels, of which no mention had hitherto been made by anatomists.

After assigning various reasons for the defective result of the injections of former anatomists, Moreschi came to the conclusion, that his researches had been crowned with a degree of success, for which all his predecessors in the inquiry had in vain longed; and that they were calculated to throw much light on the true structure of the organ, which he conceived had been hitherto quite misunderstood. To show that he did not by this assertion go beyond what was actually the truth, he has examined with much care the descriptions of former anatomists, from his countrymen Mondini, Achillini and Berengario, to De Graaf and Swammerdam, who, in his language, adorned this Sparta more zealously than their predecessors;—to Ruysch, whose injections proved that the cavernous bodies and spongy substance had no communication, and Heister, who, after many dissections, concluded that the body and glands of the urethra were composed of cellular texture;—and lastly to Cuvier, Roux and Blumenbach;—the first of whom only appears to have been aware of the vascular character of this organ. Though some obscure glances at this subject had been taken by such acute observers as Albinus, Santorini and Morgagni, yet, in general, the deviations from the customary doctrines on the structure of the cavernous body and outer substance of the urethra, were too insignificant to supersede future investigation, and too uncertain to be instructive. Our author, in short, considers the modern description by Fattori, author of the Italian " Guide to the Study of Human Anatomy, " as a fair specimen of the general state of anatomical knowledge on this kind of organization, and therefore regards the field open to new and accurate researches. We regret to say, that even the best and most recent descriptions, both of French and British anatomists, are either too closely copied from those of their predecessors, or contain too little original matter to exempt them from this character; and we are obliged to assent to the general truth of the assertion made by the Italian anatomist.

Such were the circumstances, when, by a favourable concurrence of events, Moreschi succeeded, as we have already stated, in unfolding the network of minute veins, which not only cover, but completely compose the glans. Those at the side of the glans scarcely exceed the vessels of the iris in size; but as those which are formed by the mutual union of the minute veins are enlarged, in proportion as they approach the centre and upper margin of the glans, and finally open into the deep dorsal vein, he regarded this as the obstacle, which prevented this part of the animal body from being examined with that attention, which is requisite to unfold the true structure of a complicated organ.

Encouraged by success so unexpected, without shrinking from the task, he proceeded to examine the subject in a different style. Having therefore injected in different ways the cutaneous or superficial dorsal vein, he examined its trunk, not far from the glans, where it is generally divided into two branches on each side. The more delicate of these are distributed on the upper surface of the prepuce, forming arches which diminish in size as they approach its extremity. The larger branches run behind the glans, where, being much diminished by supplying twigs to the sides of the prepuce at the glans, but especially at the extremity of the vascular body of the urethra, they are lost in convolutions on the frenum, where they are fashioned into fanciful and manifold turnings. Hence the body of the urethra, which has been hitherto called *spongy*, may now, in some measure, be termed *vascular*, according to Moreschi, since the portion of this body now mentioned, and the whole surface of the glans, appeared eminently vascular, from the single injection of the cutaneous dorsal vein. The rest of the dorsal vein near the arch of the pubis, filled in like manner with wax, was seen separated into two branches, one of which on the right, the other on the left, having received twigs from the scrotum and perineum below, above from the skin of the abdomen, at length inosculate with the inner saphena vein, near its union with the crural vein. (§ 32--§ 34.)

Since the body of the urethra continued empty behind, notwithstanding both injections of the cutaneous dorsal vein, Moreschi completed his experimental investigation by throwing into the bulb of the urethra itself, matter differently coloured from that which he had first injected into the anterior ramifications of the same vein distributed through the prepuce and glans. The posterior part of the urethra, therefore, being filled, the injected fluid, conveyed through the same body here and there to the *frenum*, met the contiguous ramifications, which had been already filled with wax by injection of the anterior portion of the cutaneous vein. The impulse of this variously coloured matter by different paths into the same vessels, occasioned the distension of several veins, which, rising from the sides of the bulb, are turned over the cavernous bodies of the penis, that thence they might osculate after a short interval with the veins running under the arch of the pubis. Moreschi then pursued the new and very clear proof of a direct communication of the dorsal vein with the glans, and body of the urethra, or of the origin of the same from the minute vessels of the urethra and glans, by making another trial, in which he impelled the matter, by the syringe, into the vascular body of the urethra. The result of this injection was a conclusive proof of the communication between the veins of these several parts; the glans and the cutaneous dorsal vein with its twigs creeping through the integuments of the belly, and also several cutaneous lateral branches of the penis entering the dorsal vein not far from the pubis, before this is divided and united with the saphena vein, appeared at the same time filled with fluid. Whatever he discovered by analytical trials, he thus proved more satisfactorily by the synthetic mode of investigation. (§ 35—§ 37.)

Of the small venous vessels which we have now enumerated, the greater number issue from each side of the *vascular* body of the urethra, and very few only from the bodies of the penis; and as each of them ascends from the lower or urethral to the upper part of the organ, they encircle it, and by uniting, finally form the deep dorsal vein. (§ 38.) The vessel thus formed not far from the glans, runs along the furrow of the cavernous bodies, divides into two branches beyond the pubal arch, and, supported by the cellular tissue which connects the anterior surface of the bladder to the inner wall of the pubis, descends to the sides of the pelvis. Nor is the dorsal vein the only one which is produced from the branches described, as we find in Cowper, Haller, and other writers of authority. Camper, whose researches on the nerves and vessels of the pelvis excel those of all previous writers, mentions only one dorsal vein of the penis; while Winslow thought this vein was not single, but confesses he had sometimes met with two, more rarely with three. (§ 40.)

Moreschi, who, in the trials described, discovered innumerable vessels proceeding to the vascular body of the urethra, and to the glans, does not consider it wonderful, that these chief venous trunks running under the pubal arch are not reduced to one only, or perhaps to two or three. He generally found them five in number, of which the central one is longer than the others; the lateral ones shorter, and the outer the shortest of all. (Tab. I. Fig. 2.) But in the horse, both on account of the great length of the *vascular* body of the urethra, and on account of the size of the glans, the veins increase in so great number, that, creeping extensively from the corona glandis to the symphysis, they encircle the whole penis of this quadruped. When the injection of these vessels is successful, not only the upper and lateral surfaces, but also the lower, are covered by a great abundance of veins, the calibre of which is considerable; and he therefore concludes, that every part of this organ is covered by these vessels. (§ 41—§ 42.) (Tab. III. Fig. 1.)

Further, in the ram, which is provided with a penis not shorter, considering the size of the animal, than the horse, the *vascular* body of the urethra is so slender, narrow, and thin, and the glans so little extended, that a single vein is sufficient for collecting the blood flowing from this very long body of urethra, which is so slender, that it does not exceed the dorsal vein itself in diameter. (See Tab. III. Fig. 6.) Though this *vascular* body of the ram's urethra is thin and very small, yet if properly distended with injection, it will immediately appear that it consists of nothing but a very *delicate net of vessels*, which is biparted by a line from its origin to the glans. But the network with which the glans is covered, is woven in a manner so simple and manifest, that it may be more easily conjectured, in other kinds of animals, what is its composition and the course of its vessels. Another peculiarity in the penis of the ram is, that the urethra has not the same termination with the glans, but, assuming the appearance of a cone, attached as it were to its length, is extended about two inches beyond the glans. The glans of the ram, therefore, does not appear open (*hiulca*) horizontally, nor does the urethra open at the left side; and it is a mistake to suppose that it is a sort of slender appendix; but a circular furrow only separates the base of the glans from the extremity of the cavernous bodies; and the researches of our author show satisfactorily, that the appendix now mentioned is nothing but a continuation of the canal of the urethra itself. (§ 44.)

The same arrangement may be more clearly demonstrated in the penis of the horse, on account of the greater diameter of the vessels; for in this animal the body of the urethra is divided into two parts, from which the vessels arise by a small origin. These having quitted the substance of the urethra, unite mutually by lunated arches, and decrease in number in such proportion, that, as they form larger venous vessels creeping over the whole surface of the organ, they gradually enlarge their calibre. From this arrangement of veins in the penis of the horse, Moreschi thinks it may be inferred, that the dorsal veins are multiplied either as the body of the urethra becomes thick, or as the glans is expanded in animals; so that for one single dorsal vein in the ram, they pass into a considerable number in the dog and horse.

Into the spaces necessarily formed by the origin of these vessels and their mutual union, so many productions of the cavernous bodies of the equine penis are sent, which, embracing closely the vessels now mentioned, retard the blood contained; and by this wonderful mechanism the penis is kept rigid during the process of copulation.

These vessels arising from the body of the urethra, penetrate its substance so deeply, that not the smallest particle of this body is left, which may not be regarded either as their origin or continuation.

" If we compare the series of vessels, looking to the urethral body of the ram and horse, with that which belongs to the same body in the human subject, we are compelled to acknowledge, that the improper *predicate* of this organ ought to be banished, as it conceals its true and intimate structure; and we therefore think that it ought to be justly and deservedly named, not the *cavernous body*, as formerly, but the *vascular body* of the urethra and glans. (§ 48.)

" Terms, as is well known, have been contrived for expressing the appearances of objects; it is therefore expedient that the terms should be subservient to the objects, and should follow the meaning closely.

" Since, therefore, the structure of the body of the urethra and glans is shown by observations to be very different from that which its former and improper predicate meant or implied, and which cannot communicate a just or true idea of its structure, lest the tide of custom should carry us away to the wonted style of speech, I may be henceforth allowed to call it the *vascular body*. And this name created with the object, appears to me indeed to be derived from its structure." (§ 50.)

To the question, whence is derived this universal abundance of vessels, of which the glans is composed, unless from the body of the urethra, our author answers, that it cannot be denied that there is found between the vessels composing the body of the urethra, cellular tissue, which furnishes support and connexion to its vessels; but it does not therefore follow, that this cellular body should be called cavernous, since the testicles for example, the liver and kidneys, though they are composed in a manner not unlike, of vessels and cellular tissue, yet are never called cellular organs. He further censures the barbarous taste, in which this structure has been called *parenchymatous*,—a term which, says he, although still used occasionally by some inaccurate authors, in intricate investigations on structure, yet it is difficult to see what is its signification. "It were indeed to be wished," says Moreschi, "that the practice had not become so general, of naming all things almost in this art by Greek words, expressed in Latin characters, rather than by terms either truly Latin, or at least transmitted from the golden age into Latium." (§ 51.)

In the human subject, the skin of the glans has the appearance of a most delicate membrane; in most other animals also it is very thin; but in the horse it is sufficiently thick and firm to admit of removal by dissection. Now, if after proper injection of the vessels of the penis of this animal, the integuments of the glans be stripped by art, it will be easy to see that the whole urethra, both outwardly and inwardly, consists of vessels, which are so much the more exposed, as the extremity of the urethra for about one-half inch remains free from the rounded end of the glans before the lunated hollow, by which it is surrounded at the upper part.

But, since these vessels, united so closely to each other, leave no membranous interval, and exhibit the same constitution under the glans, as may be seen openly, when the cylindrical body of the horse's urethra is divided below longitudinally, to expose its inner surface, it hence is truly proved, that the whole canal of the urethra, not less than its body and glans, is entirely *vascular*. (§ 53.)

The vessels issuing from the sides of the urethra, and conveyed through the cavernous bodies, from the lower to the upper surface of the steed's penis, must enter the dorsal veins with so much more difficulty, as the cavernous bodies impelled for copulation are more protruded and swelled; for then, on account of this swelling of the penis, the calibres of the vessels are diminished, and their flexuosities are increased; and this change, at the instant of copulation, necessarily retarding the blood flowing through them, must certainly greatly contribute to support the glans, which in the horse being much expanded, opens near the centre with an oblong chink. While this chink gives issue to the cavernous bodies which are extended to its apex, for supporting the glans, it embraces the end of the cone of the same bodies by its own fringe (limbus.)

Since the arrangement and adaptation of the vessels of the human penis is similar, it is easily understood, that the swelling (intumescence) of this organ depends, not on compression of the dorsal veins, but on contraction of the muscles, and impeded passage of that blood which delays passing through the vessels of the urethral substance, until these vessels, that they may be united with the dorsal ones, are compelled to describe a more extensive semicircumference round the cavernous bodies of the penis swelled during copulation. Moreschi concludes, therefore, that the whole vascular body of the urethra, and its continuation, namely the glans, ought entirely to swell without any effusion of blood within cells. He, however, does not absolutely deny this compression of the dorsal veins, which Albinus conceived, but admits that it may be enumerated among the effects at least, if not the causes of erection. (§ 57.)

From this it easily follows, that the structure of the cavernous bodies of the penis is not of the same sort as that of the vascular body of the urethra, as was improperly thought by all anatomical writers.

The cavernous bodies of the penis, Moreschi is most anxious to show, are endowed with a scanty number of blood-vessels, and are constructed very much of a tenacious cellular tissue. Further, in some animals,—for example, the horse, bull, and ram, this cellular tissue is so circumstanced, that the most delicate matter of injection can penetrate only a very short space to its cells. (59.)

The body of the urethra, on the contrary, abounds, beyond other organs of the human frame, in vessels chiefly venous; that is, in one uninterrupted *plexus* of very delicate venules, proceeding from the bulb to the crown of the glans, by which these, in ascending the lateral and upper surface of the penis, mutually unite in order to form the middle dorsal vein. Our author, therefore, concludes, that though this body has been examined by dissectors to the present day with various results, and he has personally succeeded in completing only two admirable preparations during the space of five years, yet, as he has explained all the objects which he proposed in the beginning of his inquiry, the body of the urethra is to be regarded as rising into a taper bundle of vessels, totally different from the cavernous bodies of the penis.

This inquiry into the structure of the urethral substance is succeeded by a short statement of observations and reflections on the structure of the spleen, or rather on the arrangement of the splenic bloodvessels. We do not regard these observations as either so original or so instructive as those to which we have already adverted; they are rather diffuse, and, as too often happens with such inquiries, a good deal mingled with matter which is merely hypothetical. We shall, however, do our best to condense them, and present merely the necessary parts of the inquiry in this place.

Anatomists who have studied the disposition of the spleen and its vessels in the lower animals, are aware that they are liable to considerable varieties; and though it is perhaps, in the present state of our knowledge, impossible to explain all these varieties in the individual tribes or genera in which they occur, yet, by a comprehensive view of them, it is reasonable to expect, that they may be more clearly understood than they would otherwise be. Our author, in this instance, adopts a principle which Malpighi first distinctly announced in physiological researches, and which, though mentioned in our author's previous dissertation, is, in our opinion, more forcibly applicable to the subject before us. The principle to which we allude is this. In the case of organs, the structure and functions of which are involved in obscurity in one tribe of living beings, if it be found that, in other tribes, the structure is so varied as to approach more closely to a perfect organ, the properties and uses of which are so well marked as to be recognised after suitable examination, and to be so important that they could not be omitted without serious injury to the frame of the animal, it may then be inferred, that organs of the kind to which we allude, are to be viewed as forming integrant parts of a great scale or arrangement which passes through many tribes, orders, and classes of animals,—that though various forms of such organs are retained for the sake of uniformity, and occasional deviations are observed, they are rarely to the extent of complete exclusion of such organs. According to this principle, it is also inferred, that, though the uses of such organs are not very conspicuous, or are absolutely unknown, in many tribes of animals, yet as they are evident, or at least not completely unknown, in others, some degree of these uses must prevail in all animals, and the precise extent of it is to be conjectured only in individual tribes.

"When I first observed," says Moreschi, "that the chief branches of the splenic artery in the horse and similar animals do not penetrate the spleen, but the stomach, I was moved with so great wonder, that I could not refrain from investigating the matter more carefully, and diligently extracting the reason of so great a difference, if possible, from the secrets of nature. My hand being set to the task, I conceived my object to be accomplished, by comparing this arrangement of vessels with the various number and situation of these vessels in different orders of animals, which embrace sometimes the œsophagus, sometimes the stomach, now the upper, now the middle, now the lower part of the intestinal tube; since it would be quite evident why certain kinds of animals, impelled and influenced by a singular force of those dissolving fluids which are secreted by such vessels, changed in the circumference and surface of the intestine at one time into *villi*, at another into glandules, sometimes into both, are never satisfied with food; and why other kinds of animals, by too great scarcity of the same fluids, either drown in long sleep, or are almost consumed with fasting, the chief property of life, that is, nutrition, being changed by changed disposition and number of splenic vessels." § 12.

After taking notice of the resistance which is frequently made to the admission of new views of the structure or functions of organs imperfectly known, and stating that the present observations are to be regarded as a merely temporary account of his researches, until opportunity of publishing a more complete work shall occur, our author proceeds to enumerate the principal facts regarding the structure and appearance of the spleen in the following manner.

"The spleen, of substance not very hard (firm), of purple colour verging to violet, is situate at the upper part of the abdomen in the left hypochondre, beneath the false or vertebral ribs, between the blind fundus (large end) of the stomach and its larger arch and the diaphragm. Not of the same size in all. To glance at its figure, it appears to approach nearest to the elliptical, uneven where it is directed to the diaphragm and ribs, flat or concave where it looks to the stomach. Often, however, its shape varies, and it may be divided into sinuosities and globules, so as to give it the appearance of a cluster of grapes (racemus).

"It is retained in the site allotted to it by several ligaments or connections; 1st, loosely to the diaphragm by means of a portion of peritoneum; 2d, to the stomach by the middle blood-vessels called short (vasa brevia), and by the omentum; 3d, by this means also to the transverse arch of the colon, where this intestine touches the left hypochondre. The spleen, nevertheless, is not entirely immovable; for when the stomach is empty, it fills with blood, and descends under the false ribs; and when the stomach is filled, the lower part especially of the spleen, is elevated against the ribs, and it becomes shorter; so that the bulk of the spleen observes a sort of inverse ratio to the fulness of the stomach.

"This organ is further covered by two membranes, one of which is a continuation of omentum, which is provided with some fat, and may easily be detached or stretched from the organ; the other, a prolongation of the peritoneum, very thin, and wonderfully adherent to the spleen, cannot be detached from it without being torn into fragments. We admit this to be the proper membrane of the spleen, which being drawn off, and, at the same time, the minute vessels being torn out (for its structure is so very soft that it may be disturbed by the slightest pressure of the finger), the organ is resolved partly into vascular partly into cellular structure. It may also be resolved into this, showing large cells, if, on applying a syringe to its artery, we inject water to wash out all the blood, and inject air and dry it subsequently. When this is done, in whatever manner it is cut, it exhibits cells; and though these are factitious, or occasioned by the experiment now mentioned, yet they have been regarded by several judicious persons to be natural and peculiar to the organ. Further, the cells which appear in the spleen of the larger quadrupeds are not to be regarded as true cells, but as the sections of so many vessels abounding in the thick membrane derived from the peritoneum. The spleen also, in animals of this kind, undergoes so great pressure, that, without this provision, it must be seriously injured." § 16.

The splenic artery derived from the cœliac trunk has a much larger calibre than appears consistent with an organ of its usual size; for though the spleen is smaller than the liver, the artery which goes to it is six or seven times larger than that which goes to the liver.

The splenic artery, on reaching the spleen, divides into three or four principal branches, from each of which a small artery proceeds upward to the large end of the stomach, to form what are named the short vessels (vasa brevia), or rather the *spleno-gastric.* The principal branches, on the contrary, after running along the surface of the organ, at length enter its substance, and, in their progress, are divided into branches, constantly diminishing, in such manner however that the twigs of one branch have direct and manifold communication with those of others. The extremities of these small arteries at length terminate in so many vascular prolongations or *pencils* (penicilli), which communicate by inosculation with small roots or origins of veins. These again gradually unite and coalesce into vessels, which increase in size as they recede from their roots, and finally emerge from the flat surface of the organ, in the form of four or five considerable veins, which unite and form the splenic vein. According to Moreschi, the blood which is conveyed through the branches of the splenic artery to its small divisions, is returned from this organ by the splenic veins, almost unchanged; it is more fluid, coagulates with greater difficulty than the blood of the portal vein, and is of a more vivid colour, being similar to arterial blood. He then proceeds to notice the disparity between the proportional size of the hepatic and splenic arteries, when compared to the organs to which they belong, and concludes that the blood conveyed by the splenic artery cannot be intended to nourish that organ—that all of it is not destined to the spleen; but, since both the spleno-gastric vessels and the left gastro-epiploic derive their blood from the same source, the splenic circulation, in general, is intended as a sort of reservoir and regulating standard of that of the stomach, (21, 22); and this splenic blood, in his opinion, while in the arterial state, supplies the stomach with whatever is necessary to the process of digestion; while that which comes from the splenic vein is useful in augmenting the fluidity of the blood conveyed by the portal vein to the liver.

To the natural objection, that if the stomach require more blood than is conveyed by its proper vessels, why are not these vessels larger or more numerous,—he judiciously answers, that the condition of this organ and its capacity are not at all times the same in the same individual; and as there are seasons during which much blood in the gastric vessels would be injurious, so there are others in which it is both useful and necessary. He then proceeds to state, that, as in most carnivorous and voracious animals the splenic vessels are large and numerous, and as this is connected with an abundant secretion of gastric fluid, it appears reasonable to think, that this disposition of the splenic vessels is a contrivance employed to conduct the process of digestion in these animals with facility, and without inducing disease in the circulation of the stomach. It is somewhat like a confirmation of this inference, that in the horse and similar animals which, as Moreschi observes, are able to digest by a single stomach a quantity of vegetable matter which requires four in the ruminating animals, the splenic artery, running along the upper margin of the spleen, furnishes the coats of the stomach with numerous vessels, which become more abundant as they pass on, and are, with the exception of a few small branches to the spleen, finally lost in the stomach.

Our author then proceeds to derive similar conclusions from the phenomena of digestion, and the disposition of the spleen and its blood-vessels in various orders and kinds of birds, compared with what he terms lethargic animals, by which he appears to understand several of the amphibious tribes, as the tortoise, lizard, frog, &c. (27); and, after answering those objections which might be conceived to be derived from the structure of the molluscous animals and insects which are destitute of spleen, and even some vertebrated animals in the same circumstances, concludes with the inference, that the spleen is not an auxiliary organ to the liver; that in animals in which the latter is sufficient to the purposes of digestion, the former is wanting, and that its main purpose is to regulate the circulation of the stomach,—to be subservient to the preparation of the gastric fluid,—and to act as a reservoir for blood, without effecting in it those changes which take place in its transition from arteries to veins.

In the third essay in this work, on the gravid womb, we expected to find some new and instructive information on the structure or arrangement of vessels in the placenta. We cannot discover, however, that Moreschi has been more successful in the prosecution of this inquiry, than any of his predecessors; and we find actually nothing added to the sum of what was already known on the process of fœtal growth and uterine or placental circulation. He states, in his 32d paragraph, what is sufficiently well known, that injections of the vessels either of the mother or of the fœtus, penetrate the placenta to a certain depth only; that those of the fœtal vessels never reach the maternal or uterine side of the placenta, nor do those of the womb reach the vessels of the fœtus; but he nevertheless infers, from the phenomena of hemorrhage, that a communication does exist. We need scarcely observe, that this argument has no force whatever.

In his 37th paragraph, Moreschi further informs us, that in several repeated injections he never succeeded in impelling either the most delicate glutinous matter, or mercury, from the arteries of the umbilical chord into the corresponding vein, or from this vein into the arteries through the placental substance. As the injection from the arteries however filled all the vessels belonging to it almost instantaneously, he concludes, that between the venous roots or origins of the chord, and the minute arteries of the womb, there is the same connexion which subsists between the venous twigs of the maternal vessels, and the arterial branches of the fœtal side of the placenta; "from which," says he, "it may be inferred, that the vessels of the umbilical chord are equal in number to the uterine vessels, which mutually inosculate."

These appear to be the most remarkable parts of the short disquisition before us. In other respects, it exhibits a correct and sufficiently comprehensive view of the present state of knowledge of the uterine, fœtal, and placental circulation.

The work is accompanied with four large engravings, exhibiting delineations of the preparations made by Moreschi, and employed in his descriptions or reasonings. They are executed with the accuracy and neatness for which Italian engraving has long been distinguished.

We cannot close this work without adverting to a circumstance which may to many appear both uninteresting and unimportant, but which ought not to be passed over in silence in a work composed in the country which gave birth to Tully, and boasted in its patronage of Quintilian, and was distinguished for first reviving the almost extinguished embers of ancient Roman literature. We daresay our readers will anticipate us when we inform them, that the three disquisitions before us are written in the Latin language; but we regret to say, with so many violations of what we have been taught to regard as the most classical and correct form of that language, that unless we were actually told of it, we could not believe that it was the production of an Italian professor, but rather of some heavy Batavian, of thrice blessed memory, whose attempts to express himself in the language of ancient Rome have never enabled him to make a nearer approach to the style of Cicero and Celsus, than merely to substitute, almost word for word, his native Bœotian in Latin terms. Independent of the confusion which never fails to result from a parenthetical and involved mode of description or detail, and from an attempt to crowd into a single sentence half a score at least of different facts or arguments, most strangely hinged on each other, the very words and expressions are not unfrequently extremely ungrammatical. We have not particularly sought for errors of this description; but they were spontaneously thrust on us in numerous places. The following examples may be mentioned. There is no such word as *dorsuales*, which is uniformly used for *dorsalis*. *Nedum* (42, 13) for *non solum*, is improperly used to express *not only ; much less* is its proper signification. *Magnâ diametro*, in the same member, has no meaning whatever. The author evidently wishes to say, " that the inferior surface is also covered by a great number of such vessels, of considerable calibre." *Rete* (44, 13), which is used in the ablative, joined with *subtilissimo*, is totally unsanctioned by classical use, which uniformly makes the ablative case of such words in *i* (*reti*). We have repeatedly observed the same error to be committed by many modern writers of Latin, and even by Haller. *Vasculare* is used erroneously, *passim*, for *vasculosum*. *Efforment* (45, 14) and its participle *Efformata* (46, 14), are rather affected expressions, and, we think, not sanctioned by authority. Instead of the latter, *Enata* would certainly have been more correct. The 52d paragraph, 16th page, is a most miserably constructed sentence. It consists first of a condition, in which are involved other conditions; and then of consequences, to which other consequences are attached; and it may, on the whole, be presented as one of the most perfect specimens of confused and involved writing which we have ever had the good fortune to decypher. In the 57th paragraph, *propterea* is used for *igitur*, in a most unwarranted, and, we are disposed to think, a very affected manner. *Splen*, and *stomachus*, which are repeatedly used for *lien* and *ventriculus*, are Greek, and must always be regarded as barbarisms, where a Latin term can be found. *Elementum* (25, 10), though adopted by the poets and philosophers, when writing on the philosophy of the ancients, is never used by classical authors for the *sea*. *Tetigimus* (40, 18) is improperly used for *submonuimus*. The most extraordinary error, however, which perhaps any author pretending to write in Latin could commit, is the positive violation of the grammatical rules and just idioms of the language. Several of this description have occurred to us; but two are so flagrant that they must be mentioned. The 3d paragraph of the 2d dissertation commences with the following words—" Animalia autem gramen vel aliud vegetabilium genus pascentia," &c. Now, we defy the author to produce a single example, from any classic of authority, in which *pasco* is ever used in the sense of *eating*, intransitively; and the sentence ought to have stood—Animalia gramine vel alio vegetabilium genere vescentia, &c. In the same manner, *processu* is found, in the true spirit of modern Latinity, for the present participle *progredientes*, or some such word. It is with some degree of pain that we feel obliged to expose these failings in the Latin style of a Bolognian professor; but the evil is so general, that we doubt not it may ultimately terminate in the modern authors forming a new and unknown species of Latin, which will be just as like the models which they pretend to copy, as the monkish barbarism of John of Holywood, or the fantastic Arabesque of Gaddesden. If Latin is still to be used in medical writings, it ought at least to be free of barbarisms and positive errors; and if this cannot be done, it would be much more rational, and not less creditable, for the author to express himself in a modern language generally understood.

VI.

Franz Daniel Reisseisen, uber den Bau der Lungen, &c. Berlin, lei August Rucker. 1822.

Francisci Danielis Reisseisen Medicinae Doctoris Argentoratensis de Fabrica Pulmonum Commentatio a Regia Academia Scientiarum Berolinensi Praemio Ornata. Latine expressit Justus Fredericus Carolus Hecker, Medic. Utr. Doctor in Universitate Litteraria Berolinensi privatim docens. Cum Tabulis VI. aeri et coloratis. Berolini, apud Augustum Rucker. 1822.

Memoire sur la Structure du Poumon de l'homme, &c. &c. Par M. Magendie, Journal de Physiologie Exper. Tom. 1.

In the year 1808 the Royal Academy of Sciences at Berlin bestowed its prize on Francis Daniel Reisseisen of Strasburg, for an essay on the structure of the lungs. In consequence of the unsettled state of public affairs, it was impracticable to publish engravings of the drawings which then accompanied this essay; but as soon as this was possible, the Academy delegated to Rudolphi the task of preparing for publication, engravings of those anatomical preparations which were requisite to explain the researches of the author. Various obstacles, however, appear to have delayed the editor or the artist, and it was only in the summer of 1822 that the work appeared in the form in which we now find it. The commentary was originally written in the German language; at present the original is accompanied with a Latin translation by Justus Frederic Charles Hecker, a private medical teacher in the University of Berlin. It consists of 26 pages large folio letterpress, most beautifully printed on excellent imperial paper, and is accompanied with six copper-plate coloured engravings of the objects demonstrated by the author. The execution of these engravings is distinguished by that delicacy of delineation and colouring which characterize the anatomical engravings of the foreign artists, and in which, whatever advantage British artists possess over them in engraving other subjects, they have long been pre-eminent. The researches of Reisseisen cannot be said to have added much to the sum of anatomical knowledge, nor to have unfolded many facts, which might be termed discoveries; but they have shown more positively than formerly, what is known and what is not known, what is certain and what is doubtful, in our knowledge of the pulmonic structure, and have placed its peculiarities in a clearer light than formerly. Reisseisen, indeed, himself disclaims the prospect of actual discovery, and limits the qualifications of a prize-essay to more moderate though perhaps not less useful purposes.

"I only avail myself," says he, "of a suitable occasion of submitting to the judgment of the Academy certain researches on the structure of the lungs, which have been conducted by me for several years; for I trusted that this would throw light on some doubtful points. I did not regard it as expedient to comprehend in my commentary, points which were the subject either of discovery or of discussion. For the Academy are aware of the state of anatomical inquiry, and in proposing a public question, have chiefly in view to determine what is doubtful, to throw light on what is obscure, and to fill up chasms in the literature of the subject, that they may be progressively and steadily carried to expected perfection. The Academy, therefore, require that the questions proposed by them be examined with the utmost care; and that the proofs of their answers, be without omission brought forward, supported on experiments which deserve credit and are fairly narrated. In writing this Commentary, therefore, I have followed my own dissections only, and have adduced no experiments of other inquirers, unless they were supported and confirmed by my own."

That our readers, however, may form their own judgment, we shall restrict our account of this work to a simple detail of the author's researches and conclusions, in the same order, as nearly as possible, in which he has himself delivered them. Any observations of our own shall be reserved to the conclusion of this article.

The questions proposed by the Academy, with the view of obtaining satisfactory information on the structure and organization of the lungs, are five in number, and are stated in the following order.

1. How, and where does the cartilaginous tube (of the windpipe) terminate? Does it pass into the pulmonic cellular (filamentous) tissue, and assume its nature; or have its cartilaginous tubes certain limits? Or is it continued into the cellular tissue in such manner that it retains its cartilaginous character even in the extremities, or not?

2. Do the bronchial vessels pertain to the cartilaginous breath-tubes exclusively, or to the pulmonic cellular tissue also; do they nourish the former only, or the latter at the same time?

3. What are the terminations of the pulmonary artery? Do they convey blood, through the medium of the cellular substance, through the lungs only, to be returned entirely by the pulmonary veins; or do they exhale into the cellular tissue a vapoury fluid, to be discharged by expiration, and do they discharge also a vapour on the outer surface of the lungs, or not?

4. What are the origins of the pulmonary veins? Do they rise from arteries only, or from the breath-tubes, the cellular substance and outer surface also of the lungs performing the function of absorbing vessels?

5. How do the branches of the 8th pair (pneumo-gastric nerve) and intercostal nerve terminate? Does the bronchial plexus issuing from the 8th pair terminate in the bronchial membrane only? or is it distributed in the cellular substance also? Is the 8th pair united with the twigs of the cardiac plexus, which proceed from the intercostal nerve to the minutest vessels of the lungs?

I. To determine the first question, the extent and disposition of the bronchial tubes, and of the membranous surface in which they terminate, are examined in various modes. The bronchial tube and the several branches into which it divaricates is cut open and traced as far as the scalpel can follow them. The author justly describes this surface as formed by a mucous membrane, smooth, very delicate, dense, and impermeable to air, which commencing in the throat or jaws (fauces), forms a canal everywhere shut, at first single, afterwards dividing as it descends into more numerous branches, each of which is distributed through the lung in such manner, that, after forming a manifold partition, it terminates in blind or unopen extremities, while, at the same time, the capacity of the lower branches decreases in the proportion of their increasing number and smallness.

From this simple inspection, it may, according to our author, be easily concluded, that the individual vesicles or breath-cells on the surface of the lungs are the blind or shut ends of the extreme tubes; and that these, distributed in great number, constitute that mass which, spongy as it is, appeared to many anatomists a cellular structure. That this is an erroneous opinion, he infers from the following experiments.

1. Let a bronchial tube of the lobule of a calf's lung be accurately inclosed in a ligature, which is to be drawn tight after the whole lung has been inflated by the windpipe. The result is, that the whole lower part, consisting of bronchial tubes arising below the ligature, will retain the air most steadily, while the upper will collapse as the air gradually escapes,—an event which may be accelerated by incisions in various parts of the lung. Now, if the pulmonic tissue really resembled that of cellular structure, the free communication between one cell and another would gradually occasion the escape of the air from the inflated portion and its consequent collapse.

2. A quantity of quicksilver poured through a tube similarly tied into its minuter divisions, does not escape from the tied portion, into any other part of the organ, nor even by its gravity does it find its way into other empty lobules, though it is placed uppermost, or aided by slight pressure.

In answer to an objection which might be urged, that the lobules, or portions of lung supplied by individual bronchial tubes, may be separated from each other by membranous envelopes, Reisseisen replies, that such envelopes do not exist, and that all the parts of the lung being mutually united by cellular (filamentous) tissue, in which the vessels run, are retained in their position, and each lobule is united to the contiguous ones by its interposition.

3. The cellular or filamentous web of the lungs is very differently affected from the bronchial terminations, by experiments of this kind; for air blown into it by a small incision of the pulmonic pleura, between two lobules, will be slowly dissipated, and will spread gradually under it through their interstices, at one time by filling small unequal cells, at an-

other, larger cavities; nor can it be easily squeezed out, when the inflation is interrupted, but resembles a true emphysema.

These arguments the author regards as sufficient to show the nature of what is named the pulmonic parenchyme, and to refute the opinion that it is merely cellular tissue collected into small masses. He censures particularly the experiments of Helvetius, who examined the lungs after being distended with wax, or inflated, or even dried, when it is obvious that the smallest air-tubes can neither be examined in a mould of wax, and that, when dried and distended, they must be less easily seen by the eyes. On the observation of this author, also, that the lungs cut in various directions exhibited cells of uncertain shape—larger and smaller, he justly remarks, that the air-tubes cut variously by such sections must give, in different parts, circular or oval sections, larger or smaller ones, according as a larger or smaller tube is divided transversely or obliquely. To show the peculiarities of these tubes, the following directions are to be observed.

Let a pulmonic lobule be taken in the human subject from the acute margin, where the lobules are so extenuated that their uttermost ones are single, and in the lower animals from the slender appendiciform lobes; pour quicksilver into its bronchial tube, and, laying it on a level surface, urge the metal forward till it is felt in the uttermost margin of the lobe. The columns of quicksilver may then be distinguished, by means of a simple magnifying glass, proceeding into minute tubes, which grow sensible, slender, and so much shorter as they are more remote from the upper partitions, but increasing in number and density until the quicksilver at the margin is reduced to halved globules, projecting under the pleura. The result of this experiment is shown in the first figure of the second engraving.

This structure will be still more clearly demonstrated by intercepting a lobule of the same kind between two glass plates, and submitting it to the microscope. The plates must be compressed to such extent, that the quicksilver, repelled by the residual air, may be forced into the uttermost terminations of the tubes. It will thus appear, 1*st*, that the small canals are divided into branches, even to the uttermost edge, in a certain definite order, which is most uniformly observed, and that the calibre of these tubes decreases as this branching goes on; 2*d*, that the division into minute branches (*ramuli*) becomes at the end so much more frequent as each sends forth, from all parts, new branches, which, when filled with quicksilver, have the appearance of little knots (*noduli*); and, *lastly*, that their terminations become so short, that they have the appearance of halved globules, like the stalks or stems of a cluster of colewort. This is represented in the fourth figure of the second engraving.

To obviate any objection arising from the distension of the delicate pulmonic tissue by the weight of the quicksilver, and the optical illusion thence resulting, our author instituted another experiment. A very recent lung, removed from a tender animal (that of the calf, he thinks, is best), he ordered to be submersed in water and set aside. After several days, when it had well collapsed by the escape of the air, though some lobules still retained a little, it was covered with warm water, with the intention of rarifying the air, and thus distending the canals in which the air was still contained. When a small column of air was urged, by the aid of the scalpel, to the utmost terminations, the same distribution as formerly appeared, unless that the tubes being less stretched, resembled more exact cylinders. (2d and 5th figures of 2d engraving.) It is further easily perceived, that the minute canals continued to the end terminated in blind extremities or pulmonary vesicles, by submitting such lobule between glass plates to the microscope, in such manner that it may be transparent by the reflecting plate, and that, on occasional slight agitation, the air may be urged at one time forward, at another into lateral branches. Nor can the form of these vesicles cause them to be mistaken either for distended sacs, or globular bells attached to the ends of the canals, as their compass or circumference most evidently bears the same relation to the small branches from which they issue, as that of the branches does to the trunk.

These experiments, Reisseisen thinks, show abundantly, that the breath-passages of the lungs are tapering canals, blind (cæci), or shut at their extremities, membranous, or consisting of the mucous coat of the windpipe, which is obviously impermeable to air;—by which he evidently understands, that air, though admitted into these canals, does not pass through the pulmonary mucous membrane into the subjacent tissue. That such structure of canals, however, be fitted to receive and return the air, and follow readily the motions of the chest, he conceives it to require support, in order to continue pervious,—and general mobility, so that at one time it may be lengthened, at another time shortened, and its diameter may be enlarged or diminished as need requires. This purpose, he conceives, is accomplished in three modes.

1. The cartilaginous structure of the windpipe is well known to be most useful in retaining that tube constantly open; and when

the bronchial tubes penetrate the pulmonic substance, they continue to be supported by cartilages, which, though not in the annular form, suffice to prevent the tubes from being compressed, and their cavity obliterated; and as these cartilaginous plates become thinner and fewer as the bronchial tubes become more narrow and slender, they are found only in the form of halved rings, where the small branches divide; and even this arrangement cannot be traced in tubes of half a line in diameter.

2. Wherever the bronchial tubes are embraced by cartilaginous rings, muscular fibres are connected with both extremities of each cartilage. But where the cartilages lose their annular figure, the whole circumference of the canals are enclosed by muscular fibres, inserted into the cartilaginous plates, in such manner that they partly rise above them; nor do they end where the canals are destitute of cartilages, but may be traced by means of a lens, though much smaller, as far as the bronchial tubes can be opened with the knife, principally in the lungs of robust and well-grown subjects. That these muscular fibres are continued to the ends of the bronchial tubes, our author infers from their uninterrupted course to the extremity,—from the necessity of a contractile agent, at once in the large tubes and in their minutest extremities,—and from the experiments of Warner,* in which not only irritating fluids or vapours, but even mechanical agents, applied to their extreme surface, were followed by constriction, which, he conceives, could depend on the contraction of muscular fibres only.

3. Reisseisen further states, that the air-tubes are provided with peculiar fibres, which have the faculty of shortening these tubes when stretched. He first remarks, that the ligamentous texture with which the bronchial cartilages are covered, in consequence of its elasticity, possesses this property in an eminent degree; and then proceeds to show, that the proper fibres to which he more particularly alludes, form a whitish elastic tissue, situate at the posterior part of the mucous tunic. In the larynx, he observes, it is possible to perceive through the mucous membrane whitish shining fibres, which, collected into bundles, descend backward into the windpipe, where they are perpendicular to the cartilages, and behind, where these are wanting, being gathered into bundles, they are so connected with the muscular layer, that they allow its fibres to be seen through them, (Tab. 1. *b*). Where the windpipe divides into *bronchi* they are crowded more closely, and raise the mucous tunic into folds, with which they are continuous, (Tab. 1. *a*). Lastly, Where the cartilages cease to appear, arranged along the whole

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* Mem. de la Societé Roy. de Medec. ann. 1779, p. 394.

compass of the tubes like bridles, they run to the remotest ends of the small branches. In the human subject, they are so small that they can scarcely be traced in the minute breath-canals; but in the lower animals, as they are much firmer and whiter, and more abundantly interwoven with the tubes, they are easily distinguished in the smallest branches, any one of which submitted to the microscope presents the same structure evidently as the largest. Other observations further show, that these elastic fibres are continued to the bronchial extremities; for a lung taken from a recent carcase is still filled and distended with air, but soon discharges it gradually, while the air-tubes collapse. Any fluid also, injected into the wind-pipe, flows out in an uninterrupted stream; and even when a mass of wax is impelled into the pulmonic vessels, part of it escaping into the air-tubes speedily flows into the wind-pipe mixed with air. These results Reisseisen ascribes to the elastic power of these whitish fibres, as the muscular property cannot have any operation after death.

On the nature of this tissue, our author has not made up his mind. He does not regard them as purely aponeurotic, as they do not present the character of this tissue, especially in the human subject; and they are woven with such a multitude of minute arteries, that in lungs properly injected with wax, especially if the bronchial tubes (*bronchia*) be slightly inflamed, they are not whitish, as might be expected, but actually red. Their arterial structure is also easily seen by the aid of the microscope. They appear, therefore, to be a peculiar kind of fibres, to be classed with the muscular or fibrous (proper) tunic of arteries, and the structure of the human womb.

They are so thoroughly interwoven with the mucous membrane, that they were described by the early anatomists as peculiarly pertaining to it; they may be detached from it, however, by careful dissection, as Helvetius showed in his Memoir.

From these several observations and experiments our author draws the following inferences:

1st, The pulmonic breath-tube or wind-pipe, is divided into branches, which decrease in diameter and increase in number in a fixed and uniform ratio, till they terminate in extremities which are shut, and rounded.

2d, It has no communication with the pulmonic cellular tissue, nor does it assume its nature; but retains its peculiar structure to the uttermost terminations, by which being shut it resembles air-cells, or air-vesicles.

3d, It continues cartilaginous only as far as the delicacy of its structure admits cartilage; it afterwards becomes membranous.

II. To determine the extent and distribution of the arteries termed bronchial, our author employs sundry arguments derived from examination of these vessels, aided by the microscope, dissection, and injection. As these arteries, though they go to all parts of the lung, are however most easily observed in the neighbourhood of the bronchial tubes, he proceeds to trace the bronchial branches to their terminations, and then to examine the lateral twigs which go to the other parts.

After the trunks of the bronchial arteries have encircled these tubes variously, they give to each individual branch small arteries, which accompany them to their extremities, where they are so extenuated, that it is rarely possible to propel the colouring matter into them, and the microscope generally shows it stagnating not far from the extreme vesicles. (Tab. III. 1.) Often by means of anastomosing vessels subsequently to be noticed, the colouring matter is urged into the pulmonary arteries;—the ends of which being filled, may impose on the inexperienced observer, unless the distribution into branches like a tree, and their greater diameter, showed them to be pulmonary vessels.

From these trunks issue numerous small branches (*ramuli*) which, after various circuitous turns, sometimes near, sometimes more remote from their origin, penetrate the fibrous sheath of the bronchial tubes, advance somewhat through it, at length enter the mucous membrane, in which they branch into an incredible multitude of capillary vessels. In a lung which is either inflamed or well injected with red matter, the inner (mucous) surface of the bronchial tubes appears entirely red to the unassisted eye. Our author always found this very thick net of vessels to lie along the elastic fibrous substance above described, while the corresponding veins pursued rather a transverse course according to the direction of the muscular fibres.

Beside the bronchial branches and many lateral tubes, the bronchial arteries send vessels to the other parts of the lungs. For, previous to the connexion of the bronchial tubes with the pulmonic tissue, they give off considerable branches, which running beneath the pleura in notches of the lungs, give out on each side many twigs, which carried here and there, and variously bent, finally pass into a net of capillary vessels to be afterwards described. Similar branches of the bronchial artery variously distributed in the intervals of the pulmonary lobules, pour fluid into the filamentous tissue, until produced to the surface they also terminate in the same vascular net-work.

Lastly, Some twigs go to the bronchial glands, others to the coats of the pulmonary vessels, and a third set to the pulmonary nerves.

The vascular network already mentioned, appears to our author to consist of innumerable capillary vessels with common orifices, mutually interwoven, and embraces the whole surface of the cellular or filamentous tissue of the lung. Notwithstanding this mode of formation, it cannot be seen by the mere eye in healthy lungs, nor in opening living animals, in which it must be expected to contain as much blood as is proper to it,—are injections capable of showing them, unless the matter used be of the most delicate nature. It is in the state of inflammation only that Reisseisen has observed these vessels with any facility, and he therefore recommends their demonstration to be attempted in inflamed lungs only.

The vessels of which it consists, are derived from three sources; 1. small branches (*ramuli*) of the bronchial artery, with occasional accessions from the pharyngeal; 2. small branches of the pulmonary artery; 3. small branches of the pulmonary vein. From the network thus formed, proceed exhaling vessels, by the fluid from which the pulmonic pleura is kept moist. This he conceives to be established by the ordinary experiment of injecting warm water by the pulmonary veins, from the larger branches of which it quickly escapes, and moistens the surface of the lungs.

When coloured matter is injected into this network, the colouring matter is left in the vessels, while the colourless fluid escapes to the surface. Isinglass, on the other hand, when injected in the same manner, penetrates the minute passages (pores) as it were drop by drop; and is soon after distinguished in the form of an infinite multitude of minute points (*punctula*) dispersed over the whole surface of the membrane.

From these results our author infers that this vascular network is chiefly appropriated to the process of exhalation; and that the orifices by which the exhaled fluids are prepared, belong to this set of vessels. For though the pulmonary vessels are well injected, the surface of the lungs (pleura) will never be moistened, unless the fluid passes into the vascular network; and when thick fluid (*humor crassior*—lymph or serum?) is effused between the lungs and pleura by inflammation, the minute vessels of the net are reddened with adventitious blood, and give forth vessels to form what are called false membranes.

The veins, by which the blood of the bronchial arteries is returned to the venous trunks, present an anomaly, which, in the opinion of our author, has been hitherto either utterly disregarded, or looked on as an exception to the general principles observed in the arrangement of the mechanical instruments of circulation. He alludes to the well known fact, that all the small veins which correspond to the bronchial arteries in the greatest part of the lung, do not unite to form corresponding venous trunks, but terminate in the pulmonary vein through its entire course. The general arrangement is observed in a small extent only of the organ;—in that part of the bronchial tubes which is contiguous to the root or entrance of the large blood-vessels, and in part of the superficial network. The minute veins of this part unite into a common but very small trunk, termed the bronchial vein, which terminates either in the azygous vein, or in some adjoining branch of the large vein (vena cava), or in that venous trunk itself.

Similar small veins described by Gunz, derived from the vascular net, terminate at the back part of the lung, in the lower intercostal and the œsophageal veins. In the other more remote parts of the organ, however, all of them are received by the pulmonary vein, not by anastomosis as might be imagined, but by direct passage or entrance.

When matter of different colour, therefore, is injected at the same time into the bronchial artery and pulmonary vein, not only the inner surface of the bronchial tubes, but the vascular network also is found to be tinged with both matters, and the roots of the minute veins may be seen inserted into the branches of the pulmonary vein. This is most distinct in the thin margins of the pulmonary lobules, where the pulmonary veins advance to the outer surface, and evidently receive the small veins issuing from the vascular net. Lastly, if matter of a third colour be injected into the bronchial vein already mentioned, the lung will be tinged with both these colours with which the pulmonary and bronchial vein are distinguished, so definitely, that the direction and termination of the bronchial vein may be easily recognized. The distribution and presence of this vein however is not uniform; sometimes it is directed only to the bronchial glands and great vessels at the root of the lungs; in other instances it is totally wanting, and all the small veins enter into the pulmonary sinus at the root of the lungs, as has been remarked by Winslow, * Haller, † and Hildebrandt. ‡

The course of the small bronchial veins described by our author is on the contrary uniform, and, according to the result of his dissections and preparations in the lungs of the lower animals, is to be regarded as the standard of nature. Our author, therefore, regards it as a mistake to suppose, that the bronchial vein corresponds in distribution through the whole course of the lung, to the bronchial artery; and that the motion of the blood in these vessels differs from its circulation in the lungs. He concludes with the following inference:

The bronchial vessels belong not to the cartilaginous breath-tubes only, but are continued as far as they proceed, and

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* Tome iv. p. 116. 120, 121. † Fasciculi iii. p. 54. ‡ Disert. de Pulmone, p. 25.

therefore to the breath-vesicles; nor do they convey nourishment to the air-tube alone, but to all parts of the lungs, and at the same time contribute considerably to the process of secretion, which is constantly going on at the surface of the lungs.

III. The examination of the third question is resolved by our author into three separate topics; 1. The mode in which the pulmonary artery terminates; 2. The nature of its communication with the minute branches of the pulmonary veins; 3. The question of its termination in the transparent or enveloping membrane. The direct transition of arteries into veins is most clearly established in the lungs, and the notion of intermediate cells must therefore be placed among the fictions to which the ancient anatomists occasionally resorted. It is satisfactorily established by the result of numerous experiments mentioned or recorded by Haller, that fluids can be readily propelled from an artery into veins, without anywhere exceeding the capacity of the vessels; but as it was ascertained that these fluids were conveyed into the bronchial tubes either with equal or greater facility than into the veins, several anatomical and physiological authors doubted whether the arterial tubes actually passed into veins.

Fluids are conveyed into the veins more readily in a recent lung, and into the bronchial tubes in one which has been long kept. The means which Boerhaave employed to obviate this difficulty, was eventually unavailing; for though occasional insufflation to imitate the motion of breathing, was at first attended with the expected result, yet injected fluids afterwards escaped as usual by the breath-tubes (bronchi).

After these preliminary remarks, our author proceeds to examine more minutely the peculiar nature of this communication or transition of arteries into veins. The pulmonary artery accompanying the bronchial tubes to their extremities, supplies each of the vesicles with individual branches, which dividing into numerous twigs, mutually anastomosing, are distributed on the surface of the vesicles, and being again collected into a small bundle form the origins of veins. This arrangement is most easily observed in the slightly inflamed lung of an infant of two or three years. When the artery is injected with isinglass containing finely levigated ceruse, which will pass into the veins, quicksilver is to be poured into the bronchial tubes, and a minute lobule being placed between glass plates, will show the results which our author obtained. As the same phenomena are observed after injecting the pulmonary veins with isinglass, our author regards the opinion of Bichat of a capillary network intermediate between the veins and arteries as the most natural.

He further states, that this network has minute mouths, by which a fluid separated from the blood is poured into the bronchial tubes. This he thinks is established by the fact of injected fluids escaping into the bronchial tubes more readily than into any other part, and by the argument derived from microscopical observation of injected portions of lung,—that these fluids could find their way into the bronchial membrane by no other channel. The question whether these minute mouths or orifices are to be regarded as exhalant vessels or inorganic pores, he thinks cannot be determined by sensible observation, though the greater facility with which fluids pass through them, as the lung is less recent, shows that they possess during life some property of contraction, which is diminished or destroyed in consequence of death.

The third topic which our author investigates, is the question of secretion on the outer or pleural surface of the lung. When the pulmonary artery is well injected, not only the branches of the bronchial artery, but the superficial net of vessels, is filled as well as if the bronchial artery itself had been injected. This result depends on the fact, that the pulmonary artery, near its termination, sends to the superficial vascular net numerous minute vessels, which run among the air-vesicles, and anastomose with the minute extreme branches of the bronchial artery and pulmonary veins. Reisseisen always found, however, that matter of injection was conveyed from the pulmonary artery into the bronchial, with greater facility than from the bronchial into the pulmonary artery.

The inferences with which he closes this part of his inquiry are the following:

1*st*, The pulmonary artery, in the extreme vesicles of the breath-tube, terminates in a net of vessels, which form numerous and manifold anastomotic unions.

2*d*, This vascular net-work conveys the blood into the veins in an uninterrupted course.

3*d*, At its union with the minute veins, it pours fluid into the bronchial tubes, by organic orifices.

4*th*, And, by its unions with the bronchial artery, it contributes to the secretion, which goes on at the outer surface of the lungs.

IV. In the examination of his fourth question, on the origins of the pulmonary veins, the author justly employs the facts and arguments already used in showing the terminations of the pulmonary artery. He then enters into a disquisition of some length, to show that the minute veins of the lungs do not absorb, and that this process is performed in the lungs, as Hunter, Hewson, Cruickshank and Mascagni, laboured to prove by lymphatic vessels only. Though it is not superfluous to remind our readers, that Reisseisen's investigation was performed at a period before the researches of Magendie or the function of absorption were known, yet it must be admitted that his arguments have considerable force, so far, at least, as the mere mechanism of absorption is concerned. He justly remarks, for example, that though the injection of fluids into the pulmonary veins seems sometimes to show that the veins of the outer surface of the lungs have absorbing mouths; yet the structure of the breath-vesicles, and the vascular net-work already described, shows that this is an error. For, as the arteries terminate directly in minute venous vessels, it is obvious, he remarks, that the latter can have no open mouths to absorb; and fluids injected by the pulmonary veins must be effused from their extremities. Lastly, he says, if other arguments were wanting, the capacity of the veins shows that they do not absorb; for they would then require to have their calibre occasionally enlarged,—which, however, does not appear to take place, as it is well known that the calibre of the veins is much smaller in the lungs than in any other part of the body.

V. In treating of the nerves distributed to the lungs, he asserts, that Haller has committed a considerable error in stating the nerves to be neither large nor numerous. According to the researches of our author, they belong either to the breath-tubes, or to the blood-vessels. The former accompany chiefly the branches of the bronchial artery, between the coats of which they are said to run obliquely, and terminate in an inexplicable manner on the bronchial mucous membrane. During the latter part of this course, they cannot be traced by the knife, but may be recognised, by the aid of the microscope, in the smallest bronchial tubes. The nerves which go to the vessels, either encircle them, or accompany only without touching them. The former send minute twigs to the neighbouring filamentous tissue, in which they are soon completely lost, without the possibility of being traced. The latter set of nervous filaments, which accompany without touching the blood-vessels, are said partly to go to the superficial network of capillaries, partly to the bronchial glands. The researches of the author show, that all these nerves are derived from the eighth pair only, and that within the substance of the lungs, no branch of this trunk unites or anastomoses with any of the intercostal nerves, though this union is frequent on the outer part of the organ.

After this examination of the fifth and last question, the author concludes with observations on the general facts to be established by his researches and arguments, and on some physiological conclusions which flow from them. We do not perceive that these are either so instructive or so original as to require further notice of them here; and we trust our readers will consult the work itself for more minute details. On the whole, this Essay is not to be regarded as containing much new or interesting information, or as communicating to those conversant in researches of this kind, any thing with which they were not already acquainted. But it has shown the foundations on which our knowledge of the minute structure of the lung rests; and it has indicated, with greater precision than we have yet seen, the exact length to which we have advanced in our knowledge of the intimate structure of the lungs.

Before taking a final leave of this work on the structure of the lungs, it is incumbent on us to notice the ingenious observations of M. Magendie, which were published in his Journal of Experimental Physiology three years ago.[*] To study the structure of this organ, M. Magendie deviated considerably from the usual procedure of exposition and preparation; and instead of injecting, inflated the lung partially with air by the orifice of a bronchial tube, and retaining it in its distended state by means of ligature, allowed it to dry in the open air, or before the fire.

When a lung thus prepared is thoroughly dried, it is nearly transparent, and may be cut into thin slices by a sharp instrument. If a slice of this kind be placed between the eye and the light of a taper or lamp, or merely in clear sunshine, it is easy to recognise the vesicles, or what Magendie rather terms the cells of the lung. These cells observe no regular shape, do not appear to have membranous walls, and appear to be formed solely by the last divisions of the pulmonary artery, the roots of the pulmonary veins, which are immediately connected to the divisions of the artery, and lastly, by multiplied anastomosis of all these vessels.

In a recent preparation, it is impossible to see the breath-tubes going to these cells; they evidently stop when they have reached the lobe to which they belong. The mucous membrane appears also to stop at the same limit, at least it can never be traced to the vascular divisions of which the lobule principally is composed. All the cells of the same lobule mutually communicate, but they have no connexion with those of the neighbouring lobules.

Lastly, according to the researches of Magendie, the pulmonary cells vary in size at different ages, and in consequence of disease. In early life they are small and contracted;—in old age they become large and capacious. In consequence of this change, the lung of the aged becomes specifically lighter in a very considerable degree than that of the young; and from a comparison of the lung of an infant of a few days, and that of a person of seventy years, Magendie computes, that a given volume of the latter weighs fourteen times less than an equal volume of the former. It does not appear, however, that the author has made any attempt to verify this calculation by actual experiment.

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[*] Journal de Physiologie Experimentale, par F. Magendie, Tome I. pp. 78, 79.

The cells are also much larger in individuals who have coughed much, than in others of the same age whose lungs have been sound. If these individuals are aged, the cells are sometimes two lines in diameter, without manifesting any trace of rupture. In certain old subjects in whom one lung only was diseased, the sound lung, when inflated and dried according to the method above-mentioned, resembled light froth.

This change in the pulmonic tissue of the aged, which the author considers as a variety of rarefaction, he ascribes to the diminished number of capillary vessels by which the blood issuing from the right ventricle enters the pulmonary veins;—in other words, to the diminished surface on which respiration, or the conversion of venous into arterial blood, is performed.

The results of these observations do not agree in every respect with those of Reisseisen; and the attentive reader will easily recognise discrepancies, which are perhaps to be regarded as proofs, that our knowledge of the ultimate structure of the lungs is neither minute nor accurate. Personal observation would lead us to say, that what are termed *breath-cells*, or vesicles, are merely the ultimate terminations of the bronchial tubes mutually intersecting; that they are formed chiefly by a thin membrane, which is believed to be mucous, rather because it is continuous with the membrane of the *bronchi*, than because its structure or nature admits of direct demonstration; and that the outer or attached surface of this pulmonic membrane is connected to the other organic tissues of which the lung consists, by a delicate modification of that substance which has been called *cellular membrane*, but which would be more correctly named the *filamentous tissue*. It is by the same sort of tissue that the transparent pleura is attached to the lung; and when this thin membrane is carefully detached by the forceps, its structure, and the mode in which it surrounds the last divisions of the bronchial tubes, may be easily studied and recognised. It is our own opinion, however, that the subject still admits of investigation, and, in the hand of a diligent and successful observer, might be illustrated with greater precision, and more original information, than has yet been communicated.

VII.

Anatomisk Beskrivelse over Et ved Nogle Dyr-Arters Uterus Undersögt Glandulöst Organ, &c.

An Anatomical Description of a Glandular Organ observed in the Uterus of some of the Lower Animals. By H. GARTNER, M. D. Military Surgeon, Member of the Royal Medical Societies of Copenhagen and Edinburgh. Copenhagen, 1822.

OF all the animal functions, none has given rise to so many theories, or caused so many discussions, as the function of generation. Notwithstanding, however, the laborious researches which this portion of the animal economy has occasioned, few parts of physiology are less understood. Theories have succeeded theories; each has had its admirers and defenders, and each has successively sunk into oblivion to give place to the reigning favourite of the day. That the female, as well as the male, secreted a seminal fluid, is a very ancient doctrine, and one which, after having been long and keenly contested, has now fallen into discredit.

According to the theory of Hippocrates and Galen, the female semen secreted in the ovaries, or, as they were styled, until the period of Steno, the female testicles, passing through a duct on each side into the uterus, was there mixed with the male semen, and formed the rudiments of the fœtus. The observations of Harvey and Malpighi, together with the discovery of the Graafian vesicles, or ova, in the ovaries, overturned the system of the ancients, and destroyed the belief in the female semen. Caspar Bartholine, however, described a body resembling the prostate gland of the male, which he discovered under the crura of the clitoris, and De Graaf mentioned a glandular substance that surrounded the urethra, and opened into the vagina. From these a fluid, resembling the secretion of the prostate gland, was thought to be produced, and thrown out during the venereal act.

Later anatomists seem to have lost sight of this part of the apparatus of generation, and have confined the business entirely to the impregnation of the ova, and their transmission into the uterus, through the influence either of the semen itself, or of a peculiar exhalation, to which the name of *Aura Seminalis* has been given. Any fluid, which may be produced in the act of generation, is considered, according to this theory, as being formed by the mucous follicles of the vagina.

Some researches have lately been made into the anatomy of these parts in some of the lower animals, which, if confirmed in the human subject, may contribute to throw some light on this obscure part of the animal system.

Dr Gartner, an ingenious and zealous anatomist and practitioner in Copenhagen, has lately obtained an honorary medal from the Royal Society of Sciences of that city, for a discovery in the genital parts of the cow and the sow. This discovery was announced in our Journal some time ago; and we then promised to recur to it as soon as we should receive a description on which we could rely. As Dr Gartner has lately printed an essay on the subject in the Danish language, we shall endeavour, for the benefit of our readers, to compress into a short compass the information which it contains.

The author was engaged in an investigation of the lymphatics on the uterus of the cow, when, by accident, he discovered a canal full of a clear, yellowish fluid. From its appearance, he was convinced that it was neither a blood-vessel nor absorbent; and upon cutting into it, he found that he could inflate it both above and below the opening. Upon further investigation, he found that traces of the canal could be seen to within an inch of the ovary, and that, on following it downwards, it opened beside the orifice of the urethra. Having experienced some difficulty in fully developing the organ in the cow, he proceeded to search for a similar part in the sow. Here his researches were immediately crowned with success.

"The sow has a double uterus, without any neck, properly so called. That part, however, of the vagina which lies nearest the place where the uterus is divided, has prominences within on the one side, which fit into corresponding depressions on the other, and in this manner shut up the openings into the uterus. The vagina is somewhat long, and has a round figure, though a little flattened on each side. The urethra opens into it about two inches from the external parts of generation."

The first uterus which the author examined, was that of a sow three years old. Upon the paries of the vagina, which adjoins the bladder, the author felt a round, hard body, somewhat like an artery to the touch. This he opened, and dissected upwards, and for some inches he found it to be a single canal. By degrees, however, it branched out into a body that had the appearance of a gland, and resembled very much the pancreas. After giving out branches to this body for about two inches, it became again a single canal, though very minute, and stretched towards the uterus, or rather cornu of the uterus, as the uterus of the sow is destitute of a body. In order to see how this duct opened into the vagina, the author injected quicksilver through a part of it, which he had left unopened, and which lay hid under the muscle of the bladder. The mercury passed into the vagina, and he perceived that the duct terminated in a small opening close beside the orifice of the urethra.

" I have," says the author, " examined many uteri, in a pregnant as well as unimpregnated state, and the uteri of those animals that had been deprived of their ovaries, and I have generally found the same result, namely, a canal which begins on each side of the place, where the vagina terminates in the cornua uteri, passes through a glandular body in the middle of the vagina, goes under the sphincter vesicæ, and perforates the vagina close beside the orifice of the urethra."

A similar duct was found on the other side of the vagina, which followed exactly the same course as the one above mentioned.

Dr Gartner was sometimes able to trace the duct much further than is here described; he has even succeeded in following it to within an inch or two of the ovaries. In general, however, it could not be distinctly traced further than the place where the vagina terminates in the uterus. In these cases, however, small knobs or glands were seen in the tract of the duct.

The author contrived, though not without difficulty, to inject the whole canal with quicksilver.

" Before the canal is injected with mercury, it has a white and compact appearance, and seems to be about as thick as the barrel of a pen. When filled with mercury, it appears less compact, and becomes twice as large in diameter. Upon cutting into it, it has somewhat the appearance of a *vas deferens,* though not quite so compact in substance, or if I may use the expression, so cartilaginous." p. 15.

The organ is much more complicated in the cow, and the author accordingly experienced greater difficulty in developing it. The uterus of the cow is distinguished by a long and thick neck, and two horns that proceed from the body, and terminate in the Fallopian tubes. As far as we have been able to discover from the description before us, assisted by the plates, the organ in the cow consists of three parts.

1. A duct on each side of the vagina, which, beginning by a small orifice near the mouth of the urethra, immediately expands into a *capitulum* or sac, large enough to admit the point of the finger. This part differs altogether in structure from the rest of the canal, for its inner surface is smooth and polished, and its outer is fibrous and somewhat muscular, while the inner surface of the rest of the duct is composed of a mucous membrane, the outer consisting of the common cellular substance. From this part the canal runs on the paries of the

vagina next the bladder, and terminates about an inch from the os uteri in a *cul de sac*, which communicates by a valvular or cribriform opening, with the canal that begins at this part.

2. A number of follicles or cysts, on which numerous blood-vessels are ramified. These cells are generally full of a viscid fluid. Sometimes little glandular knobs are found mixed with them, and sometimes hydatids are found adhering. In a cow 16 years old, Dr Gartner found on one side a canal of a spiral form, and on the other a number of cells, under and among which ran the duct. The cells were nearly as hard as cartilage, and large enough to admit the point of the finger.

3. Under and among the cells, a canal is observed, at first of a tortuous or spiral form, which, running along the neck of the uterus, follows the curve of its horn, upon which it extends to within an inch of the ovary. This canal in the cow is pervious, from its beginning, to the place where it turns towards the cornu uteri. It can, however, be traced to within an inch of the ovary.

It is always a matter of some difficulty to describe an anatomical structure without referring to plates; we hope, however, that our readers will understand the description now given.

The author, with great candour, confesses that some of the older anatomists have partly described this organ; he even refers to the different authors that have mentioned it. We have taken some trouble in tracing the germs of this discovery in the older authors, and we have found, that, with the exception of Malpighi, the accounts which they give are so contradictory, and the description of the part so confused, that little is to be made of them. Malpighi, however, in his letter to Sponius, * has given a very clear description of the duct in the cow, from its opening in the vagina to near the curve of the cornu uteri; but beyond this, he does not appear to have traced it. He also, in some measure, describes the follicles and cysts mentioned by Dr Gartner.

Upon the whole, therefore, although this cannot be considered altogether as an original discovery, we think that the author is entitled to the praise of having given a clearer description of the part than has yet appeared, and of having followed the anatomical investigation further than has hitherto been done. The medical world is also, in our opinion, much indebted to him for having revived the memory of investigations, which, if pushed to their full extent, may yet lead to important views in physiology. It cannot however be doubted, that much remains to be done before we can come to any certain conclusions.

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* See pp. 26, 27, 28.

VIII.

A short Treatise on Operative Surgery, describing the principal Operations as they are practised in England and France, designed for the use of Students in Operating on the Dead Body. By CHARLES AVERILL, Surgeon, Cheltenham, Fellow of the Royal College of Surgeons, London. London, 1823.

THE object of the author in this little work is to describe the several steps which it is requisite for the surgeon to observe, in performing on the human body any of those operations which the cure or the removal of local disease may render necessary. It is evident, that the author has studied Operative Surgery with considerable attention, and is intimately acquainted with the most recent improvements in the art; and his Treatise may, on the whole, be regarded as a useful compendium of practical instructions, and a tolerably faithful guide to the surgical student during his labours in the dissecting-room.

Mr Averill commences his work in the most elementary manner, with rules for the performance of the simplest incisions, and those operations to which the character of *minor* has been properly applied. The reason of this is obvious enough to all who admit, that the most complex operations consist in a great degree of those which are simple, and that success in the former not unfrequently depends on dexterity in the latter.

He then proceeds to describe the method of securing arterial trunks in the proper situations for disease, wound, or other injury; and gives a very correct view of the remarkable improvements, which this important department of operative surgery has recently undergone. To tie the most trifling artery in the human body requiring ligature, is an operation so mechanically neat, is suggested by pathological principles, so well understood and so uniform in their action, and demonstrates so irresistibly the advantage and necessity of accurate anatomical knowledge, that it is now the most elegant and scientific part of surgery; and we regard it as of the utmost consequence to the surgical student to have the whole stock of modern information thus collected into the small compass before us. In the space of 23 pages of this small volume, Mr Averill has described the most approved methods of tying any one of the following arteries; radial at the wrist, ulnar at the wrist, ulnar at the middle of the fore-arm, radial near the elbow, brachial near the elbow, and at the middle of the arm, axillary artery, subclavian above or below the clavicle, carotid artery; posterior tibial, 1st, at the ankle, 2d, below the middle of the leg, 3d, high up in the leg; popliteal artery, femoral below the middle of the leg, anterior tibial, femoral artery in the thigh, or in the groin, external iliac, internal iliac, and lastly, the aorta, according to the procedure of Sir Astley Cooper. In describing the rules to be observed in performing these operations, the author has generally been sufficiently minute and accurate; though in some instances perhaps it might have been expedient to state the anatomical relations of the parts divided, and especially of the vessel to be tied, at greater length than he has done. The exposition of the subclavian artery above the clavicle is stated in the following terms.

"Make an incision three inches long just above the upper border of the clavicle, beginning half an inch from its sternal extremity, or immediately on the outer edge of the origin of the sterno-mastoid muscle; continue it through the integuments and platysma myoides, taking care not to wound the vein, which is situated before the artery, crossing it nearly at right angles; having separated it from the artery, it should be held on one side by an assistant; then feel for the eminence formed by the junction of the bony, with the cartilaginous portion of the first rib, on the outer side of which you find the artery; the nerves forming the axillary plexus are situated rather behind and to its outer side." pp. 27, 28.

Now, though no person ought to pretend to expose this artery without having previously studied every muscle, nerve, artery and vein in the vicinity, with the utmost care, we conceive it would have shown the strict connexion between relative anatomy and surgery more clearly, had the author stated very shortly not only the facts above mentioned;—but the point of the first rib over which the artery lies, the site and relative position of the anterior and middle *scalene* muscles, and the other relations of the space in which the incision is to be made. The addition of a rule peculiar to this operation, viz. of bending the head gently from the shoulder, will always facilitate the exposition of the artery; and another, common to all operations in which arteries are concerned, that of using the handle of the scalpel or a blunt instrument after the necessary incisions, ought never to be omitted.

After the subject of tying arteries, Mr Averill proceeds to those of excision of the breast, bronchotomy, œsophagotomy, the various modes of hare-lip operation, excision of the lower jaw, as performed by MM. Dupuytren and Lisfranc, and the best modes of puncturing the cavities of the peritoneum, pleura, or pericardium. The only novelty which we shall notice, is the mode of perforating the last of these membranous sacs, which is that recommended by M. Skielderup, professor of anatomy at the University of Christiana, in Norway. This operation consists in removing, by the trephine, a portion of the sternum immediately below the articulation of the cartilage of the fifth rib, and then puncturing the pericardium, which may be done at this spot without wounding the pleura or penetrating the chest. It is scarcely necessary to say, that the general expediency of this operation must be regulated by circumstances, and, above all, by the moral responsibility of the surgeon.

Of the three modes of puncturing the urinary bladder,—by the rectum, above the pubes, and by the perinæum,—our personal observation leads us to prefer the first, in which we have never known, in the hands of a good operator, any of the inconveniences which have been ascribed to it. The seminal vesicles are not in the way of being wounded, with ordinary care, and it is seldom requisite to retain a cannula in the opening; for after the bladder has been once evacuated, the obstruction of the urethra, for which the operation was required, generally subsides so much, as to allow the urine to pass by its natural channel. The perinæal operation is, in our mind, the least advisable of any; for as inflamed or enlarged prostate is too frequently the cause of the urethral obstruction, which renders the operation requisite, it is almost impossible to make the necessary incisions without the certainty of wounding this organ, or the seminal vesicles, or other parts which it is desirable, in many subjects, to avoid.

Under the head of Lithotomy, the two varieties of lateral and high operation in the male, and Lisfranc's operation in the female, are described with considerable accuracy and detail. As Mr Averill has enumerated the three modes of performing the lateral operation by the knife, the *lithotome cachée*, and the cutting gorget, without attempting to distinguish their several merits, we mention the subject for the sole purpose of saying, that the first ought always to be used as the most rational and safe, and that in which the surgeon has the merit of trusting more to his knowledge of parts, and management of the knife, than to any mechanical contrivances whatever. The *lithotome cachée* is to be regarded as the most rude, barbarous, and unscientific instrument that ever was introduced into the practice of surgery, very well calculated for the hands of a mere mechanical operator, but utterly unfit for the use of the anatomical surgeon. The gorget, though sanctioned by the high authority of Sir Cæsar Hawkins in this country, and of Scarpa in Italy, is little better, and ought now to be regarded as a mere mechanical contrivance to supply the defect of correct knowledge of the parts, and of that skilful dexterity which is acquired by frequent performance of the operation on the dead subject, and habitual subsequent examination of the incisions. We indeed confidently hope, that, if both of these instruments are not already banished from the practice of lithotomy, the great attention with which surgical anatomy has been recently studied, will at length open the eyes of surgeons on the exact value of all mechanical contrivances beyond the simple knife, and convince them that the success of their operations will depend very much on the simplicity of their instruments, and almost entirely on the confidence with which they divide parts with which they are intimately acquainted. For cutting into the bladder, in this operation, there cannot be a doubt that the only instrument which is necessary beyond the scalpel, is the button-pointed knife of Sir Astley Cooper, or that employed by Mr Blizard. Both of these differ so little from the ordinary scalpel or bistoury in the beak for fitting to the grooved staff, that their simplicity and neatness will, we think, demonstrate the advantage of using them in preference to every contrivance, however ingenious, with which the surgeon's chest has been encumbered, either in ancient or modern times.

Castration, and excision of the penis, follow. In the first of these, besides the ordinary method, Mr Averill describes that followed by M. Lisfranc, that of Vincent Kern, a German surgeon, and modifications by Dubois, Richerand, and Dupuytren. The only difficulty in this operation, is to prevent the vessels from slipping within the ring; and the great object is, to tie them separately from the nerves and other parts of the chord. The best means of obviating these difficulties will be found in the finger of an adroit assistant, as Lisfranc shows, and as we have often seen. In incision of the penis, the author describes the method practised in the French hospitals, of preserving the urethra pervious, and preventing the urine from disturbing the dressings, or irritating the wound. It appears complicated and unnecessarily cumbrous. With the precaution of inserting and fixing a proper silver tube or cannula, we have seldom seen any inconvenience.

On the subject of amputation, which immediately follows, Mr Averill remarks, that " we seldom see it performed in a masterly style; some parts being cut with which the knife has no business to come in contact, or others which should be completely divided being left partially separated, till the mangling teeth of the saw tear them asunder." He then proceeds to describe the procedure in different sorts of amputation, from that of the phalanges of the fingers and toes to the removal of the arm or leg from their respective articulations. In all of these forms, the operation is to be conducted on the same general principles, and, though simple enough, will always be facilitated by accurate knowledge of the parts to be divided, or of those to be removed. For this purpose, it is well to remember, that all kinds of amputation may be referred to two general heads; 1*st*, those in which the bone requires to be divided transversely by the saw; and 2*d*, those in which the bone is to be removed from its articulation.

The first kind of amputations are the simplest, and may be performed with facility and dexterity by mere novices in the art. Next to the tying of the arteries, the only points which require particular attention are, 1*st*, to divide the soft parts in such manner that the stump may be uniform, and that the bone may be properly covered and may not protrude; 2*d*, that the bone be sawn through in such manner that it may not be splintered or otherwise injured. It is astonishing how much the mode of performing this latter part of the operation appears to be misunderstood, and how rarely is it well done even by those who in other respects operate properly. To divide a bone easily and without shattering by the saw, it ought never to be forgotten, 1*st*, That the bone itself ought to be held steadily and firmly; 2*d*, That the saw be properly set, so that it may divide and not be forced like a wedge through the bone; and 3*d*, That each stroke be performed slowly, steadily, and the whole length of the cutting edge, and not with the short, rapid and jerking motion which is too often practised, and which would disgrace the most ignorant carpenter's apprentice in the kingdom.

The second kind of amputation, or that in which the bone or member is to be disarticulated, requires greater delicacy and more accurate attention to the figure and situation of the parts. The greatest operations of this description are those in which the shoulder or thigh-bone are removed from the glenoid or cotyloid cavities;—but modern surgery has also shown the propriety of removing, in certain circumstances, portions, or the whole of the foot from the tarsal bones, or even of removing one or more of these bones, and leaving the fore part of the foot attached to those which remain. Under the head of the first of these operations, the author describes the common method practised by English surgeons, and which is nearly the same as that described by Mr Guthrie, the method of Dessault as modified by Baron Larrey, and the method of M. Lisfranc, which is at present regarded by most French surgeons as the most eligible and expeditious. It is described in the following terms:

"Supposing the left extremity is to be removed; the patient is placed on an elevated seat, one assistant pressing the artery above the clavicle on the first rib, whilst another draws the arm forwards. The operator standing behind the patient, with a long bladed catling, pierces the integuments on the inner edge of the latissimus dorsi muscle, opposite the middle of the axilla, and pushes it obliquely upwards and forwards, till its point strikes against the under surface of the acromion; then by raising the handle of the knife its point is lowered, and protruded just before the clavicle, at the part where it joins the acromion. He then, by cutting downwards and outwards, forms a flap from the superior and posterior part of the arm, including the whole breadth of the deltoid muscle, and a part of the latissimus dorsi. This being held back by the assistant, the joint is cut through by passing the knife between its articulatory surfaces from behind forwards, and a corresponding flap is formed by cutting downwards and outwards between the muscles and bone on the inner side of the arm. The vessels being tied, and the flaps placed in contact with each other, the operation is finished." pp. 134–136.

The removal of the lower extremity at the hip-joint is described according to the procedure of the same surgeon.

"The nates of the patient resting on the edge of the table, and the extremity being supported by an assistant; the operator draws a line, an inch in length, from the anterior and superior spinous process of the ilium, straight down the thigh. From this point he marks another inwards towards the pubes, of half an inch, so as to form a right angle. On the inner extremity of the last, he places the point of a long bladed catling, and pushes it perpendicularly downwards, till it strikes against the head of the femur; then, passing it on the outer side of the bone, he thrusts it onwards, till it protrudes at about an inch from the margin of the anus. He now cuts outwards, for near an inch, in order to get clear of the great trochanter, and forms the external flap, four or five inches in length, by cutting down the limb between the muscles and bone. The femoral artery, which may now be seen, is to be compressed between the fingers and thumb of an assistant; while the operator thrusts the knife in and out, at the same points as before; but carrying it on the inner side of the head of the bone, he forms a smaller flap on that side of the extremity. He then, with the point of his knife, cuts through the capsular ligament surrounding the head of the femur, dislocates the bone, and removes the limb by dividing the round ligament, and the remaining adhesions. The blood-vessels being secured, and the flaps approximated, the operation is concluded." pp. 158–160.

It is unnecessary to notice further the rules delivered by Mr Averill for performing other kinds of disarticulating amputation, or those for lacrymal fistula, extirpation of the eye, and trephining.—with which the work is concluded. The specimens which we have already given, will enable our readers to understand and estimate the utility of this little work and the merits of its author, and will probably show, that it is by no means an unimportant addition to the library of the student of surgery. In a subsequent edition, we trust the author will endeavour to see it printed with fewer typographical errors.

IX.

Observations on Air found in the Pleura, in a Case of Pneumato-Thorax, with Experiments on the Absorption of different kinds of Air introduced into the Pleura. Read before the Royal Society, June 6th, 1823. By John Davy, M. D. F. R. S., &c. Philosophical Transactions, 1823.

The interesting and instructive paper of which we propose to give an abstract in the following pages, is occupied with two distinct subjects. It contains, in the first place, two excellent cases of that singular disease, Pneuma-thorax, * in one of which the disorder was detected during life, and permanently removed by the operation of paracentesis; and, secondly, it includes an experimental inquiry on the gases occasionally found in various parts of the human body, and on the power of some of the tissues in absorbing aeriform fluids.

The first case is that of a young invalided soldier, in whom the disease was not discovered before death, because, on account of his hopeless state, the chest was not accurately examined. When Dr Davy proceeded to inspect the body, fourteen hours after death (in February), he observed that the right side of the chest was unusually full; that it sounded very hollow when struck; and that the right portion of the diaphragm was thrust down so as to be convex, instead of concave, towards the abdomen. He therefore punctured the right side of the chest under water, upon which a large quantity of gaseous fluid was discharged, and part of it collected in receivers. It measured altogether 225 cubic inches, or somewhat more than eight pints. The right lung was of course very much compressed, and it adhered to the pericardium and back of the chest. On its upper lobe, an aperture was found in the pleura as wide as the diameter of a crow quill; and through this aperture air issued freely, when the lungs were dilated from the trachea. This aperture led to a little cavity in the lung, an inch in width, the sides of which touched one another, but did not adhere; and from the side of the cavity opposed to the aperture, but not directly opposite, three small holes communicated with a large vomica seven times as large as the other, and opening into bronchial tube by a large perforation. From this account it is evident that the intervention of the little sinus between the opening towards the bronchial tube and that into the sac of the pleura would have all the effect of a valve, which permitted the escape of the inhaled air into the sac, but prevented its return. The lung was, besides, studded with tubercles, varying in size from that of a mustard-seed to that of a pea. The former were translucent, the latter of various degrees of opacity, but none had suppurated.

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* Dr Davy has preferred the title of Pneumato-thorax. We do not remember at present whether or not this is a new alteration. But as there appears to be no sound classical reason for the change, and terms of pentasyllabical longitude hardly add to the elegance of our nomenclature, it will be advisable to retain the name of pneumo-thorax, or more properly pneuma-thorax, which was first given to this disease by Itard (who first accurately described it), and has since been adopted by Laënnec and other French writers.

The water which had taken the place of the air contained about an ounce of purulent matter intermingled with it, and about 11 ounces of serum.*

The other case is that of a soldier, who was invalided on account of frequent hæmoptysis, subsequent to a severe blow on the chest, and who was seized with the symptoms of pneumathorax after a violent fit of coughing. These were a feeling of tightness in the chest and belly, protuberance of the left side, a remarkably hollow sound in that side when it was struck, and pulsation of the apex of the heart under the right nipple. The pulse was 130, and small; the breathing short, frequent, and difficult; the countenance expressive of great anxiety and agitation. His agony having continued to increase steadily for eight days, Dr Davy, after consulting with two of his professional brethren, resolved to attempt the relief of the breathing, by perforating the chest. A bladder was tied on a canula over the trochar, which was then thrust into the chest, and withdrawn from the canula into the bladder. In this way the air was collected for analysis. The first attempt to evacuate the air was unsuccessful; only five cubic inches were discharged. But, nowise discouraged, Dr Davy repeated the operation next day, and with perfect success. "On withdrawing the stilette into the bladder, a large quantity of air rushed out, and distended the bladder; and, on separating the bladder from the canula, air from the chest continued to rush out with violence, as from a blow-pipe." When the gas ceased to issue, and the air began to enter the chest on inspiration, the canula was taken out, and the wound closed. The operation was followed by great and immediate relief; and three weeks and a half afterwards, when the author's paper was drawn up, the man was better than on his admission into the hospital; the cough troubled him little; he could lie on the left side, which had been impossible for several months before the operation; the left side was much less tympanitic and protuberant; and Dr Davy indulges the hope of his ultimate recovery. The heart, however, continued to beat on the right side, and the fluctuation of a fluid could be felt in the left side of the chest.

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* The pus fell to the bottom, when the fluid was allowed to rest 24 hours. The quantity of serum was estimated by the weight of the residue of a small portion of the water.

These are two cases of the commonest variety of pneumathorax, namely, that in which the air proceeds from a communication between the bronchi and sac of the pleura, through means of a suppurated tubercle opening into both. Such at least was unequivocally the nature of the first; and such too we are disposed to consider the other to have been. It must be regretted that in the latter case the ingenious author did not avail himself of the diagnostics employed by Laennec and his followers, through means of which its exact nature would have been at once determined. The presence of air in the sac of the pleura is known by the concurrence of a hollow sound on percussion with the want of the sound of respiration through the stethoscope: * and if a communication exists between the bronchial tubes and the air in the pleura, there is conveyed to the ear through the instrument, when the patient *breathes* or speaks, or coughs, a peculiar impression termed the *Tintement métallique*, that is, a thrill or quivering, like what is produced by striking gently a cup of metal, glass, or porcelain. †

This, we believe, is one of the first cases in which the operation of tapping has been resorted to for the express purpose of removing pneumathorax. Several times indeed air has been withdrawn from the chest during the performance of paracentesis for empyema; Laennec himself has even given two cases in which air was ascertained before the operation [II. 134. 174. Obs. 39. 43] to be present in the cavity, and was removed by the canula; but in these, and many other similar instances, the operation was resorted to for the empyema, not for the pneumathorax.

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* Laennec, Ausc. Méd. i. 449. † Do. —— ii. 91.

The rest of the paper relates to the nature of the gases found in various parts of the human body, and the relative power which the animal tissues possess of absorbing various aëriform fluids. The remarks made on this subject are rather desultory, and neither so complete nor so precise as could have been wished. But they are nevertheless of great interest, and the author's experiments tend to establish some new and rather unexpected facts respecting absorption. His observations, (to which he was led by the composition, soon to be mentioned, of the gaseous fluid of pneumathorax) may be considered, in reference to physiology, under two heads: under the first, we shall arrange his researches on the absorption of gases by the different membranes of the body; under the second, those on the gases disengaged in certain circumstances from the blood.

1. On analyzing the aëriform fluid collected from the chest of the first case of pneumathorax, he was surprised to find, that it did not contain a particle of oxigen, but consisted of azote and carbonic acid only, in the proportion of 92 of the former, and 8 of the latter. These results were singularly confirmed afterwards by the analysis of the air withdrawn in the second case by puncture during life. It consisted of 93 azote, and 7 carbonic acid.

He was therefore naturally led to inquire, whether this gaseous fluid was originally atmospherical air, which had escaped from the bronchi into the sac of the pleura, or a peculiar effusion; and subsequently, to ascertain the changes which atmospherical air and other gases undergo when introduced into the sound pleura. For these ends he first injected atmospherical into the pleura of a dog, closed the aperture carefully, and killed the animal in forty-eight hours; when he found, as had been anticipated, a great part of the oxygen gone; the residual gas being composed of 93 azote, and 7 oxigen, (instead of 79 a. + 21 0.) with a trace of carbonic acid. Oxigen gas is evidently absorbed by the pleura much more readily than azote. It appears farther, that carbonic acid gas is absorbed less rapidly than oxigen, but more quickly than azote: A mixture of atmospheric air with a fourth of its volume of carbonic acid, and therefore containing per cent. 20 carbonic acid, 63.2 azote, and 16.8 oxigen, when allowed to remain two days in the pleura of a dog, contained in every hundred parts 18·3 carbonic acid, 78.3 azote, and 3.4 oxigen. Hydrogen gas is absorbed with the same facility as oxigen, if not more rapidly; and it is a very remarkable fact, that, although no azote exist in the gas or gases injected, it will be found abundantly in that which remains. Thus, when a mixture of carbonic acid and hydrogen in the proportion of 57.5 of the former, and 42.5 of the latter, was left for two days and five hours in the pleura of a dog, it consisted of 12 per cent. of carbonic acid, 88 of azote, and contained no hydrogen. The nitric oxide gas is absorbed with great rapidity. When 30 cubic inches of a mixture containing 44.5 per cent. of azote, and 55.5 of nitric oxide gas, were injected into the pleura, the animal died in five hours; and there remained only five cubical inches of aëriform fluid, of which only 6.9 per cent. were the nitric oxide and the rest azote. Hence, of 16.5 cubic inches of nitric oxide, only a third of a cubic inch remained. The experiment with this gas was several times repeated; and Dr Davy observes, that although serious symptoms ensued, they always went off in twelve hours; and that, if the animal was killed at this time, the whole gas was found absorbed. The blood, both in these experiments, and in the case which proved fatal, had a chocolate-brown colour; from which circumstance he infers, that the nitric oxide gas causes death by changing the nature of the blood. Dr Davy should have added, however, that this fact had been satisfactorily determined long ago by M. Nysten. The experiments of that physiologist and those of our author, it may be remarked, do not altogether correspond: For Nysten says, that one cubic inch and three-fourths (30 cubic centimètres), injected into the pleura of a dog, caused violent tetanus, and death in forty-five minutes. In fact, he considered the nitric oxide to be the most poisonous of all the gases, even more so than sulphuretted hydrogen. In all his experiments, which were very numerous, he never failed to find the blood in the state described by Dr Davy.[*] The nitrous oxide gas is absorbed by the pleura with great rapidity. Thirty cubical inches were injected by Dr Davy into the chest, along with a little atmospherical air; and, when the animal was killed twenty-four hours afterwards, every particle of the nitrous oxide had disappeared.

These experiments appear to establish, that the gases differ very much in their susceptibility of absorption by the serous membranes. The author has not attempted to discover what law they follow in this respect; nor can we pretend to do it for him, because his experiments are not comparable with each other. With regard to most of the gases, however, it appears, that the susceptibility of being absorbed approaches pretty nearly their relative solubility in water. But there is a striking exception to this rule in the case of hydrogen; and on the whole it will probably turn out, as Dr Davy hints, that they follow a law peculiar to the vital economy, and not conformable with any of their physical habitudes.

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[*] Recherches Pathologiques, p. 151-9.

Dr Davy also renders it probable, that other membranes besides the serous have the property of absorbing the different gases with different degrees of celerity. At least it is certain, that the aëriform contents of the cavities, which are lined by mucous membrane, are almost identical with the gases he found in the serous sac of the pleura. Thus Magendie and Chevreul observed, that the aëriform fluid in the human stomach was composed of 11 oxygen, 14 carbonic acid, 71.5 azote, and 3.5 hydrogen; that, in the small intestines, it consisted of 24.3 carbonic acid, 8.5 to 55.5 hydrogen, and 20 to 66.6 azote; and that, in the great intestines, its component parts were from 43.5 to 70 carbonic acid, 5.5 to 20 hydrogen and carburetted hydrogen, and 18.5 to 67.5 azote.* Dr Davy has likewise found the aëriform fluid of the intestines to consist of carbonic acid and azote; and in the frontal sinuses and maxillary antra of the sheep he found a much larger quantity of carbonic acid, and a much less proportion of oxigen, than exist in atmospherical air.

2. We have now to notice his observations on the gases, which in certain circumstances are disengaged from the blood. This subject he was led to investigate in consequence of observing the singular fact already mentioned, the evolution of azote into the sac of the pleura when hydrogen had been injected into it. He first shows, that the evolved gas could not have proceeded from air believed by some physiologists to exist between the two pleuræ. Notwithstanding what Laënnec has stated, on the authority of Ribes (i. 438), no air is ever found in that situation; and Dr Davy proves, that Ribes was deceived by a singular source of fallacy.† How, therefore, did the azote arrive in the situation where he found it in the said experiment? Was it derived, says he, from the blood as an exhalation or secretion? This question, he thinks, may be answered in the affirmative, because azote is occasionally disengaged by the blood in other situations. Thus, Messrs Allen and Pepys found that it was sometimes disengaged from the lungs in respiration; and it is a fact singularly analogous with the results

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* See Ann. de Chim. et de Phys. II.

† It is odd that a physician of such repute as Laënnec should err so egregiously in this matter. "Ce fluide aëriforme," says he, speaking of the gas alleged by Ribes to exist in the sac of the pleura, "n'est autre chose que la sérosité elle même *réduite en vapeur par la chaleur animale!*"

of Dr Davy's experiment (a fact, however, which he seems unaccountably to have overlooked), that, when another innocuous gas is substituted for the azote which is breathed,—for example, hydrogen,—then azote is disengaged from the lungs in great abundance. Another situation in which azote is disengaged from the blood is in the cavities of the blood-vessels soon after death. Dr Davy has been at some pains to show how this evolution of gas cannot be always owing to putrefaction, and, if we do not mistake, he seems to think he has been the first to call in question the capability of such a cause to produce it. We apprehend, that no pathologist who is worthy of the name, ever now dreams of attributing to the putrefactive process those bubbles of air, which are frequently found soon after death in the heart and great vessels, and more particularly in the vessels of the pia mater; and Dr Davy will find, that the same opinions he has formed have been stated before by various authors. He is the first, however, so far as we know, who has analyzed and determined the nature of this air. He had been induced to suspect, that it could not be carbonic acid, as Sir E. Home imagines; because the alkali in the blood is not saturated with carbonic acid, because serum can absorb more of that gas than water, and because he had been unable to procure the slightest evolution of carbonic acid from the blood either during its spontaneous coagulation, * or by placing it in the vacuum of an air-pump. Accordingly, having found the cellular tissue of the anterior and posterior mediastinum vesicular in the body of a young man who died of dysentery, he contrived to collect the contents of the vesicles, and found them composed of 7 oxigen, 4 carbonic acid, and 79 azote; and in another case in which vesicles were formed under the pleura on the lungs, the aëriform fluid consisted entirely of azote and carbonic acid, in the proportion of 5 of the former to 1 of the latter.

There can be little doubt, therefore, that azote may be disengaged from the blood in more places than in the air-cells of the lungs during respiration; and probably the cavity of the pleura may in certain circumstances be one of these. We may suggest to Dr Davy, however, that, notwithstanding his three reasons against the disengagement of carbonic acid from the blood, his experiments prove that it actually is disengaged. It will be a very interesting subject of future inquiry, to ascertain under what circumstances, and owing to what causes, these phenomena are produced.

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* Other experimenters have alleged, that carbonic acid is disengaged during the spontaneous coagulation of the blood.

X.

1. *A Treatise on the Physiology and Diseases of the Ear: Containing a Comparative View of its Structure and Functions, and of its various Diseases, arranged according to the Anatomy of the Organ, or as they affect the External, the Intermediate, and the Internal Ear.* Third Edition, with considerable Additions and Improvements. By JOHN HARRISON CURTIS, Esq. Aurist to his Majesty, and to their Royal Highnesses the Duke and Duchess of Gloucester; Surgeon to the Royal Dispensary for Diseases of the Ear; Lecturer on the Anatomy, Physiology, and Pathology of the Ear; and Fellow of the Medical Society of London, &c. &c. London, 1823.

2. *An Engraved Representation of the Anatomy of the Human Ear, exhibiting in one View the External and Internal Parts of that Organ* in situ. *Accompanied with a Plate of Outlines and References, with copious Explanations: To which are added, Surgical Remarks on introducing the Probe and Catheter into the Eustachian Tube by the Nostril, &c. &c. The whole designed as a Guide to Acoustic Surgery.* By THOMAS BUCHANAN, C. M. Licentiate of the University of Glasgow, Corresponding Member of the Phrenological Society of Edinburgh, &c. &c. Fol. pp. 38. Hull, 1823.

OUR attention is directed to these two works, as the most recent contributions to that department of medicine, the object of which is to cure or relieve the disorders of the organ of hearing.

The first of these is entitled "A Treatise on the Physiology and Diseases of the Ear, containing a Comparative View of its Structure and Functions, and of its various Diseases," &c. Now, it cannot be denied that this has a most scientific air, and holds forth many magnificent professions which are calculated to convey a most exalted opinion of the attainments and character of the author. Considerable address is also displayed in the "Introduction," in tracing the improvements of the healing art from the remotest ages, in showing the gradual subdivisions which have taken place, and especially that happy distribution of labour which has given individual importance to the dentist, oculist, and especially the aurist. In this ingenious disquisition, the memory of Adam Smith, and the principles of the "Wealth of Nations," are not forgotten; and in a word, every thing is done by the author to inspire the reader with a high veneration for his character, and unbounded confidence in his talents and accomplishments. Lest, however, this should miss effect, and that none may plead ignorance of the existence, professions, and merits of John Harrison Curtis, Esq., the special attention of the reader is requested to the following beautiful aphorism, and the accompanying delicate commentary.

"The mind of man, it is clear, though capacious and possessed of very extensive powers, cannot embrace the whole circle of science, or retain it with that exactness which is necessary to excel; he must select a part of the circle, if he wishes to shine, and must bend his attention to that subject alone, in scientific pursuits."

Note on this passage.

"A memoir of the author will be found in the European Magazine for April 1819, which will show he enjoyed the most ample opportunities of medical improvement. First, under his father, an eminent physician in the country, and brother to the late celebrated botanist Mr W. Curtis, and next under the different lecturers of the metropolis: after which he obtained his qualifications as a surgeon from the Royal College of Surgeons in London; and was appointed, in 1805, a medical officer in the hospital department of the public service, being employed, during the most interesting part of the late war, at Stapleton, Haslar, and Forton. Since he commenced practice, he has confined himself to this particular class of diseases."—*Introduction*, pp. xv–xvi.

This Introduction further shows, that the ear is a most important organ, that its diseases are exceedingly injurious and very little understood, but that, in the hands of Mr J. H. Curtis, the art of acoustic therapeutics has attained such a pitch of perfection, that there shall no longer be ground for complaint.

Such, in short terms, may be said to be the nature of the mere vestibule to this temple of acoustic surgery, constructed by John Harrison Curtis, Esq. Whether the interior of the building corresponds to the expectations which these magnanimous views are calculated to inspire, can be determined only by those who have the courage to explore the further results of the labours of the author.

The treatise consists of four chapters, the first of which is devoted to the subject of the structure and uses of the several parts of which the organ consists; in the other three, the author expounds the diseases to which these parts are exposed, and the manner in which they are to be treated. On the first of these we will not dwell, nor detain our readers with any account of it. It is a compilation, not very well performed, of a little comparative anatomy, a small portion of the physical part of acoustics, and occasional facts on the phenomena of hearing, with various desultory observations on the probable uses or properties of the individual parts. These several ingredients are blended together in such a happy and novel kind of disorder, that we doubt not the high opinion which the Introduction is fitted to convey of the powers of the author, will be carried to a much more sublime elevation by the first chapter of the Treatise.

In the subsequent chapters, the author condescends to adopt a mode of arrangement more like that of other people; and treats, 1*st*, of the diseases of the external ear; 2*d*, of those of the tympanum; and, 3*d*, of those of the internal ear.

In the 2d chapter, on the diseases of the external ear, after a few general remarks, we find the subjects of inflammation, vesicular, or vesiculo-pustular eruptions, termed *herpes*, morbid septum of the passage, polypous growths, inspissated wax, the general head of accidents, or foreign bodies or insects in the canal, and, lastly, congenital inspissation of cerumen, successively discussed. On these subjects, or their treatment, we cannot perceive any thing new,—not much either to praise or to blame, unless we were to speak of the general lack of information which is betrayed through the whole.

The 3d chapter, on the diseases of the tympanal cavity, comprehends two subdivisions only, puriform discharge, and obstruction of the Eustachian tube. On the description of the first, which was copied in the former edition of this work with considerable fidelity from that of Mr Saunders, we find the subsequent observations of M. Alard on catarrh of the ear, engrafted; and, after several remarks on the expediency of curing it as early as possible, and the efficacy of various remedies, the authority of M. Itard is brought forward on the subject of injections into the tympanal cavity. The only peculiarity worthy of particular notice is, that Mr Curtis has done us the honour to transcribe from our account of the work of M. Itard, without acknowledgement, as much as suited his purpose, but with the most gross errors in the management of proper names. We trust the author will remedy this grievance in his 4th edition.

The author proceeds to the second subject of this chapter, obstruction of the Eustachian tube requiring perforation of the tympanum. After some meagre and desultory remarks on the causes of this obstruction, that is, its pathological causes, Mr Curtis unfolds the principle of operating, in the following terms:

"Though this species of deafness is highly formidable, yet the cure of it has been in many instances accomplished, by means of an operation first suggested, and successfully performed by Sir Astley Cooper: to this he was naturally led by the important observation, that the sense of hearing, though imperfect, is not destroyed in cases of suppuration of the tympanum, or its partial injury from other causes: hence, as deafness is complete from obstruction of the tube, from no entrance being given to the air, he very rationally supposed that, by making a small puncture in the membrane, in order to allow the air to get access, the machinery of the ear would thus be set in motion.

" The experiment confirmed the justice of the idea; and hearing has been preserved in a number of instances in this way, not only by Sir A. Cooper, but by myself and others." pp. 113, 114.

This is succeeded by an observation of M. Deleau, to prove that the tympanum is of little or no use, and that the effect of its loss on distinct hearing is exactly similar to that of one or two of the front teeth on articulation. Mr Curtis immediately discovers, that perforation of the tympanum was not first performed in this country by Sir A. Cooper, but by M. Eli, a Parisian surgeon, " who had been in the habit of perforating the tympanum in cases where there was no paralysis of the 7th pair." (115.) The operation is then described; and the chapter is concluded with recommendations to repeat it, if necessary.

The 4th chapter, on diseases of the internal ear, by which is meant the labyrinth, is divided into two parts,—the constitutional and the local maladies. The principle of this distinction is not very obvious, at least to our perception; for the author regards as constitutional, not only the deafness depending on fever, disorder of the digestive function, worms, hysteria, hypochondriasis, &c., which are generally regarded as examples of constitutional deafness; but that which arises from any affection of the brain, as hemorrhage, tumour, &c., affections of the nerves of the face, nay, organic diseases of the ear itself, or of the auditory nerve, are regarded as constitutional. The result of this random method of distinction is, that under the head of local diseases of the internal ear, the author is obliged to enumerate several which he had previously regarded as constitutional. After stating generally, that all of these have been comprehended under the vague name of nervous deafness, he subsequently admits that they are to be " considered as modifications of constitutional disease connected with that state which constitutes the hypochondriac and hysterical habit." Now, the inconsistency of this arrangement is not diminished by telling us, " that the general morbid disposition is here extended to a particular sense;" for what is this but actually repeating what had already been stated under the head of constitutional diseases?

This novel method of explaining the principles of his art, is not however the only error which Mr Curtis has committed. It is too evident that he found himself perplexed what to say in this chapter, and completely at a loss for want of matter. After informing his reader in the introduction that " M. Itard had chiefly extended the work of Mr Saunders and my treatise, without adding much that is new or important in the treatment," (p. 35.) consistency, not to speak of regard for his own reputation as an aurist, required him to shun imitation of that writer, and above all to avoid his prominent errors. Yet the very first thing Mr Curtis does, after the abortive attempt at enumeration of local diseases, is to adopt from M. Itard, the whole of that absurd and useless division of the causes of nervous deafness (in consequence of disease of the nerve), on which we had occasion to animadvert, with what we conceived a merited degree of severity. What opinion is to be formed of the consistency, the modesty, or the judgment of such an author, who asserts that the very work, from which he borrows, is merely an extension of his own literary efforts? Surely Mr Curtis had completely forgotten the unfortunate assertion which escaped him in his introduction, otherwise he must be the blindest of mortals to the conclusion which follows. M. Itard's book is indeed a very indifferent performance, and in no part more injudicious, than in that to which we allude; but it is most egregious imprudence to adopt his distinctions,—for *a fortiori*, the work which is composed on such a model, has every chance of exceeding its errors and absurdity. The chapter terminates in a discussion on French, Spanish and German ears, for the particulars about which the work must itself be consulted.

Then follows a conclusion in which nothing is concluded, unless that the author " succeeded with the assistance and patronage of the first persons in rank, science and professional celebrity, in instituting a Public Dispensary for the Diseases of the Ear," which is now in a most prosperous condition.

Fifty-nine Cases of various sorts of acoustic disease follow; they are perhaps the best part of the work.

The second of the works before us, which assumes the imposing name of " The Guide to Acoustic Surgery," issues from a most extraordinary place, the town of Hull, on the banks of the Humber. Though the complaint of Hamlet, that " all the world's a stage," may now be converted into that of all the world being scientific, and talking science, some of our readers may not be quite prepared to expect descriptions of the structure and diseases of the human ear from a place more known in commerce than in science. The anomaly, however, is, for the credit of Hull, explained by the circumstance of the author being originally connected with Glasgow—a name not altogether unknown in the records of medicine at least.

The work consists of two parts, the first of which is strictly anatomical; the second consists of remarks on one or two surgical operations, and exhibits a synoptical view of the diseases incident to the organ.

The first or anatomical part is divided into three chapters, in the first of which the author describes the auricle or external ear, the auditory canal, and the ceruminous glands; in the second, the tympanal cavity, its bones and minute muscles, the mastoid cells shortly, the eustachian tube, and the tympano-vestibular, and tympano-cochlear orifices (*fenestra ovalis* and *fenestra rotunda*); and in the third chapter, he examines the vestibule, semicircular canals, and cochlea, and the substances which they contain. These descriptions are short, and in general correct; but they do not present any views of the structure of the organ which can be said to be new or unknown.

The chief peculiarity in this part of Mr Buchanan's work, is an attempt to represent the whole organ of hearing at a single view, in one engraving; and in this he has succeeded, as well as such attempt can be expected to succeed. Whoever knows the figure, relative position, and mutual communication of the several parts which compose this organ, is aware that it is anatomically impossible to see them all distinctly at the same instant, and in the same view;—that to obtain a correct view of one part, others are concealed or overlooked; and that the attempt of exhibiting all the parts of an organ, so differently situated, at once, is necessarily attended with an imperfect exposition of the greater number. To do the author justice, however, we must admit that one of his objects in this engraving, is to show the situation and direction of the Eustachian tube, and to regulate on anatomical principles the operation of introducing the probe or catheter by the nostril into the Eustachian tube.

This leads us to the second division of Mr Buchanan's work, in which he describes the best method of performing the operation now mentioned, and proposes to improve that for puncturing the tympanal membrane.

The last is the only part of our author's labours to which we have leisure at present to call the attention of our readers.

" The *mode* of operating has been looked upon by many, as of little or no moment; so much so, that there are no directions given in any modern system of Surgery, shewing in what manner the puncture ought to be made; and yet, it is the *mode alone* which has brought the operation into disrepute.

"It is thought sufficient to introduce a sharp-pointed instrument through the Membrana Tympani, that no injury is done to any of the Ossicula Auditus; and if the surgeon has been so fortunate as not to wound the Vascular Membrane of the Tympanum, he considers the operation as having been performed in a very superior manner. The patient connected again with the moral world by means of the *puncture*, is overjoyed at being so suddenly restored to hearing with so little pain or danger.

"But in a little time his former complaint returns; which is at first attributed to cold, and this supposed indisposition he endeavours to get rid of as soon as possible,—but medicine has little effect;—the Meatus is then inspected, and *adhesion* of the sides of the *puncture* is found to have taken place." pp. 34, 35.

Mr Buchanan then goes on to show, that the repetition of the operation, instead of effecting the purpose of the surgeon, or realizing the hopes of the patient, injures the membrane more and more, breaks sometimes the ossicular apparatus, and too often aggravates the defect of hearing, which it was intended to remove. We know not how far these apprehensions are well founded. It is certain that the operation has been much abused, and often utterly misapplied, from ignorance of the true pathological cause of deafness. But we think we have seen it attended with a beneficial result to the hearing, where the tube only was obstructed; and it must always be kept in mind, that no surgical operation can be a remedy unless it is judiciously and seasonably used, as well as properly performed.

Mr Buchanan, however, without appearing to advert to the necessity of these conditions, contends that the mode of operating by mere puncture, is in all instances founded on false principles, and thus fails to produce the benefit anticipated, or to obviate the evil which follows. To prove that this representation of surgical practice is not exaggerated, he quotes an extract from the work of Mr J. C. Saunders, in which the operation is said to consist in simple perforation only of the membrane;—and a case from the same, in which this simple perforation was performed and repeated at the interval of ten days, and yet the opening afterwards required to be enlarged by the introduction of a probe, ere the hearing was permanently benefited.

These evils arise, according to the author, from inattention to the structure and shape of the *Membrana Tympani*, "which is oval and muscular, and not only so, but that the fibres run in a peculiar direction from those of any other muscle, from the circumference to the centre."—"Hence it is," he continues, "that when the operator thought he was *cutting*, he was only *separating* the fibres by the mere mechanical power of the instrument; and that the union which took place afterwards, was an effort of nature to restore the fibres to their original position and tone." p. 37.

Now, we will not refuse to Mr Buchanan, that, if he can prove that the tympanal membrane is really muscular in the human subject; in other words, if he can show that the closure of the puncture is occasioned by the effort of nature to restore the fibres to their original place,—his method of operating may be expected to be more successful than that hitherto employed. He shall speak for himself.

" Adhesion will take place in almost every instance. But let a muscle in a similar situation be cut across, and the fibres will immediately *retract* to an extent corresponding to the size of the wound.

" The peculiarity of the mode of operation which I would recommend, is, to *drill* the perforation, and by means of the quadrangular point of the Perforator, the fibres of the Membrana Tympani will be *cut across*—retraction take place—the wound assume an oval figure, and there will be less danger of a union of the parts taking place than in the common mode of operating by a simple puncture." pp. 37, 38.

We are not much disposed to cavil with this improvement, which is what in some instances is performed by judicious operators. Such of our friends as have practised this operation, we know, uniformly caused the instrument to revolve on its axis several times, in order to obviate, if possible, the union of the cut edges.

In the synoptical table which follows, the author is not fortunate in his nomenclature, which though in imitation of Latin, is the most uncouth collection of sesquipedalian words our eyes have for a long time beheld. In other respects, however, this is a respectable performance; and if more accurately printed, may be useful to the student. Typographical errors, we presume they are, abound through the whole work; the anatomical terms are most miserably printed, with an error in every second word.

PART III.

MEDICAL INTELLIGENCE.

DOMESTIC.

Report of the Diseases of Birmingham from October 3d, 1823, *to January 3d,* 1824. *By John Darwall, M. D.*

ACUTE DISEASES.

	No.		No.
Mania Puerperalis	1	Obstipatio	49
Ophthalmia	4	Splenitis	1
Dentitio	9	Nephritis	1
Odontalgia	1	Febris Infantum	8
Aphthæ	2	Synochus	14
Cynanche Tonsillaris	6	Typhus	10
——— Trachealis	1	Arthritis Rheumatica	12
Cephalalgia	21	Rheumatismus A.	6
Pleuritis	2	Variola	9
Catarrhus	8	Roseola	1
Melæna	1	Urticaria	5
Colica	1	Erysipelas Faciei	8
Cholera	3	Abscessus	1
Peritonitis	1	Paronychia	1
Diarrhœa	10	Phlegmatia Dolens	1

CHRONIC DISEASES.

	No.		No.
Hemiplegia	5	Hæmatemesis	3
Paraplegia	2	Dysenteria	1
Epilepsia	3	Ascites	3
Paralysis Partialis	1	Peritonitis C.	1
Hypochondriasis	1	Lumbrici	4
Chorea	2	Tænia	1
Ranula	1	Hæmorrhoides	3
Surditas	1	Marasmus	12
Cynanche Trachealis C.	1	Hepatitis C.	9
Hæmoptysis	3	Icterus	10
Phth. Pulmonalis	31	Dysuria	4
Hydrothorax	2	Nephralgia	2
Dyspnœa	1	Hæmaturia	1
Asthma	42	Leucorrhœa	4
Tussis	10	Hydrops Ovarii	1
Bronchitis C.	19	Amenorrhœa	5
Morbus Cæruleus	1	Menorrhagia	10
Mastodynia	1	Dysmenorrhœa	2
Dyspepsia	119	Hysteralgia	1
Asthenia	15	Hernia Pudendi	1
Gastrodynia	7	Rheumatismus C.	27

Lumbago	5	Lepra	3
Ischiatica	1	Struma	2
Tumor ab Hydrargyro	2	Anasarca	3
Exostosis	1	Hydrops totius Corporis	5
Nævus	1	Carcinoma Uteri et Nympharum	3
Porrigo	3	Luxatio	2
Strophulus	1		
Acne	3	Total	610
Impetigo	4		

The number of patients in the last three months has been much greater, as might have been expected, than in the preceding quarter; but the increase has arisen, rather from the aggravation of such diseases as are general through the whole year, than from much addition to those which are more frequent in winter. Still there has been some augmentation in the latter, which will be readily seen by comparing the present Table with the last. More patients have presented themselves with rheumatic and pulmonary affections than during July, August, and September; but the increase principally consists in disorders of the alimentary canal, none of which, however, have been very severe. Nevertheless, the afflux of patients has not altogether been so numerous as in the corresponding quarter for the last three years; and inquiry among the most respectable practitioners has led to a similar conclusion with regard to the higher classes. Probably this is to be considered as the natural consequence of so severe a winter as that of 1822 and 1823; by which all the oldest and the feeblest of the younger patients would be cut off, who must always form the great bulk both in private practice and in the practice of a public institution. The weather during the last quarter, though variable and frequently rainy, has been generally mild, and, with the exception of two or three days in December, we have as yet experienced little cold.

Twenty-one cases of acute headache appear in the present list, most of which required general and local depletion; the former without the latter was rarely successful, while the latter alone was frequently sufficient. Perhaps local blood-letting might be oftener serviceable, if employed more liberally. The usual plan of applying five or six leeches, can scarcely be expected to be advantageous; nor is it at all surprising, that they who are addicted to this practice should have little confidence in its success. The French, who are commonly blamed for inefficient practice, may too seldom, perhaps, have recourse to general blood-letting; but it may be much questioned, whether British practitioners would not considerably modify their propensity for large general depletion, were they in the habit of ordering fifteen or twenty, instead of five or six leeches at a time. It is quite certain, that many individuals will bear blood taken from them in this manner, nay, will be materially benefited by it, who could not sustain half the quantity drawn by venesection without a serious diminution of their strength. These remarks hold equally good, if cupping be practised; for it is notorious, that the way in which this operation is usually performed, makes it perfectly inefficient and useless.

The instance of chronic croup seems to have originated with, and to have been aggravated by the employment of the individual. He was a milkman, and necessarily much exposed to the vicissitude of the weather. The complaint commenced twelve months before I saw him, and, with slight temporary intermissions, had continued till that time. His state was peculiarly distressing; great dyspnœa preventing his taking more than a few steps together. Hoarseness and a loud croupy noise. Some tenderness on pressing the trachea. He appeared to be in danger of almost instant suffocation. Small doses of tartrate of antimony and calomel, with tartar emetic ointment rubbed externally, so as to produce a very full crop of pustules, gave very considerable relief; and under the use of this, and afterwards blue pill, with decoction of sarsaparilla, he has lost all his complaint, except a slight hoarseness, and has returned to his employment.

Several likewise of the cases of chronic bronchitis originated from the respective employments of the patients. Four of the nineteen marked in the Table were occupied in shops filled with small dust. Two were pearl-button turners, and two iron and brass filers. All of them were relieved, and one of the latter very materially, by the sarsaparilla and antimonial ointment.

It has been somewhere observed, that there is a species of consumption ensuing after delivery, which, if treated with tonics, is quickly removed. I have seen several instances of this; but in one the symptoms so nearly resembled the conclusion of tubercular consumption, that I considered it perfectly hopeless. There was cough, puriform expectoration, dyspnœa, hectic fever, and colliquative sweats; yet within a fortnight this patient was restored to health, and she has since resumed her former occupation, that of a market-woman. The treatment consisted merely in giving gentle aperients, with infusion of roses and paregoric. I question not, but that in the hands of a regular or irregular quack, (for there are both), this would be cited as a successful case of tubercular phthisis. I have myself no clear idea of the pathology of these cases.

I may here also notice an affection of the bronchial tubes that often accompanies a febrile state of the system, and which to those who have not witnessed it before, must wear a very unfavourable aspect, from the great copiousness of the expectoration. It has been ranged in the present list under Bronchitis Chronica; but it may be a matter of doubt whether it should be looked upon as accompanying fever, or *vice versa.*

The symptoms are, for the most part, those of very mild fever; the patient being languid, thirsty, incapable of exertion, and suffering slight pains in the limbs. In one case the expectoration equalled a pint and a half at least in twenty-four hours. The pulse is usually slow, never quicker than in health; there are no hectic flushes, or any difficulty of breathing. In the course of two or three weeks the expectoration gradually diminishes, and at length completely disappears. The recovery, upon the whole, greatly resembles that from fever. There does not appear much danger attendant upon it.

The pulse has appeared to me a peculiarly good guide in this and other affections of the bronchi. So far as my own experience goes, the pulse, in tubercular phthysis, is uniformly small, quick, and irritable, while, in affections of the bronchi, it only becomes so towards the fatal termination, after the system has been considerably wasted and reduced. The chance of recovery is in proportion to the undisturbed state of the pulse.

Sauvages is the only medical author in whose works I can find any thing like a satisfactory account of this affection; and on this, as on many other occasions, I have obtained that information from the learned Frenchman, which I have in vain sought for elsewhere. Boerhaave, Hoffman, and Sydenham, have all noticed the occurrence of cough, and even expectoration, with fever; but Sauvages alone has given a clear and explicit account. If we except the first part of the following sentence, the description accurately corresponds with the cases, from which the above has been taken. " Post febres putridas ac malignas, corpore satis jam purgato, accidit interdum, maxime in senibus, ægrum valde debilem esse, et quandoque tussi, interdum etiam exreatu, magnam glutinosi viscosique phlegmatis copiam expectorare, quod symptoma non tantum ægro terrorem injicit, sed et ipsi medico, præsertim minus cauto, imposuit pro phthyseos prodromo." Vol. iv. p. 622.

On Contagion.—We are informed by a distinguished correspondent, that an opinion expressed in the review of Dr Paris and Mr Fonblanque's work, in our Number for last October, (see the bottom of p. 608), has led to a belief, among our friends of the Metropolis, that we are disposed to defend the doctrine of the non-contagious nature of Plague and Continued Fever. As nothing could be farther from the intention of the writer of that review, than to espouse or advocate such a cause, we beg to make the following explanation. All which was intended to be alleged is, that, with respect to those varieties of domestic fever, which clearly originate in fatigue, unwholesome food, personal filth, and the like, *and which cannot be traced to contagion,* we do not think the evidence yet collected sufficient to prove *them* contagious in their nature;—that the affirmative opinion, however, is the safer one in relation to medical police, and, therefore, the opinion which we should always decidedly act upon;—but that facts do not warrant the strong expressions employed on the subject by Dr Paris and his coadjutor;—expressions which, besides tending to suppress inquiry, cannot fail to convey to the unprofessional part of the public, unwarranted notions of the nature of contagion, and to expose an unprotected breach in the doctrine to the assaults of the anti-contagionists. But, as to the infectious or contagious nature of *some* forms of domestic fever, those, namely, which are always at least *traceable* to contagion, after what we have both seen and suffered, we should be blind and callous indeed, if we could disbelieve or doubt it. As some atonement for the misconceptions we have caused, we shall relate the following remarkable fact, which occurred not long ago in this city, and which, we think, deserves to be recorded; because, although the doctrine of contagion scarcely requires farther support, it is well to leave its adversaries, if possible, not a single inch of ground whereon to agitate themselves.

During the height of the late epidemic fever of Edinburgh, namely, from the end of 1817 to the middle of 1820, of fifteen gentlemen who successively filled the offices, (seven in number), of physicians' and surgeons' assistants, in the Royal Infirmary and Fever Hospital, and who remained a moderate length of time at their duty, one only escaped the disease. In the course of the first seventeen months, sixteen cases occurred among ten persons; that is, five had it once, four twice, one thrice; and, with a solitary exception, the fever appeared in all, and each time, in its most violent, unequivocal, *exquisite* form. Yet, during the whole of that period, continued fever was almost, we will even venture to say, absolutely unknown among other young men of their rank in society, throughout the city. The only cause of disease, to which the gentlemen of the hospitals were especially exposed, and not their equals in rank and age throughout the city, was—contagion. The inference is inevitable. Lest it should be alleged, however, (and it has been alleged by some of the bigotted and prejudiced cavillers, who stand ever foremost in the ranks of the anti-contagionists), that fatigue could have uniformly produced so peculiar a form of disease, in so many persons collected together, we may add the following particulars. In the first place, their duty, though often laborious enough, was certainly not so fatiguing as that of thousands of their rank in other professions. But, waiving that consideration, it was singular that, except two, they were all peculiarly sound in constitution and robust in frame; nine were very much above the average as to strength—five of these even athletic; and the stoutest were soonest attacked. It is almost unnecessary to observe, that their mode of living was the best possible, and that the Managers, knowing the risk they ran, paid every regard to their comforts. One circumstance only rendered them obnoxious to the disease, and accounts for so many having suffered. They were all of the age most susceptible of contagion, namely, from twenty-one to twenty-five. We have often demanded, but in vain, an objection to this fact. It was even submitted four years ago to the captious acumen of Dr Charles Maclean, who granted that it was a very strong one, and said he must take time to consider it.—These particulars we owe to Dr Christison, who was himself one of the gentlemen alluded to, having had the fever three times in fifteen months. To the accuracy of the general fact we can also bear the amplest testimony; indeed it is familiar to every practitioner in Edinburgh.

Trials for Murder.—1. In December last a very remarkable instance occurred, in the Justiciary Court, of the value and effect of medical evidence. It was on the trial of a girl, Christian Paterson, for murder and assault. The prisoner, a prostitute, one morning entered, along with several others, the house of the deceased, a brothel-keeper; and, in a fit of drunken wantonness, after tossing various kitchen utensils at the deceased, inflicted on her, with a heavy smoothing-iron, a denuding wound of the scalp, an inch and a half long. The woman was stunned, but not much, and only for a short time, and the wound bled a great deal. Little evidence was procured of the progress of her case, from the day of the accident till thirty days afterwards. At that time the wound was an open sore, she constantly complained of headach; and it was rendered very probable, that the wound had never closed, and that she had been scarcely ever free from headach after she received the blow. She had constantly kept up, however, an ancient custom of getting frequently drunk,—was seen several times much the worse of liquor, and on one occasion particularly, at a ball, about a fortnight before her death, she danced with great spirit, and drank with much earnestness, till she made herself insensibly drunk. About eight weeks after receiving the wound, she was attacked with erysipelas of the face and scalp, and died of it in ten days. No proof was led to show where the erysipelas had begun. On dissection, the ordinary appearances produced by that disease were found in the scalp and within the cranium; the wound had passed into the state of a round indolent ulcer; and a small hole, produced apparently by ulcerative absorption, penetrated the skull, opposite the middle of the ulcer in the integuments. The hole was widest at its inner end. The *dura mater* corresponding with it was not diseased, and no purulent matter was deposited between that membrane and the bone. This case is instructive in several respects. There can be no doubt, that, in an abstract point of view, erysipelas supervening upon an injury of the head, must be accounted one of its natural consequences, for which the assailant is as much responsible as for its more immediate effects. For, though not a necessary, or perhaps even a very frequent consequence, it is nevertheless sufficiently common, and may supervene under all circumstances, whether favourable or unfavourable to the patient's recovery from the primary effects of the injury. At the same time, it is one of those effects which result with much greater certainty under improper management, especially if that mismanagement consist not merely in neglect, but also in positive irregularities. In the present case, the deceased had led, in almost every possible particular, the very kind of life which (especially during the prevalence of an epidemic erysipelas) was calculated to induce the most serious consequences, even though the wound had been much slighter. The medical witnesses, therefore, declared, that the death of the deceased could not with any certainty be ascribed to the wound inflicted by the prisoner. Upon their evidence the prosecutor immediately departed from the charge of murder, and the prisoner was found guilty of the assault. But there was likewise, in our opinion, another difficulty in the case, which would have rendered it impossible to convict the prisoner of murder, though the plea of *malum regimen* had failed. The practitioner who attended the deceased from the invasion of the erysipelas, had gone to sea, and, consequently, no evidence could be led to prove where the fatal disorder began. Now, it might have been, after all, but an attack of ordinary erysipelas, commencing in the face, and quite unconnected with the wound. The diagnosis between idiopathic erysipelas, and that symptomatic variety which supervenes upon injuries of the head, is sufficiently simple. The former never begins, so far as we know, on the head, but always on the cheek or tip of the nose; the latter never but around the bruise, incision or sore produced by the injury. This is a distinction of some importance; for criminal cases like that related, are of pretty frequent occurrence. In the course of last year we met with no fewer than three in this city.

2. The following case, which occurred a few weeks ago at the Hertford Assizes, will serve to show the necessity of some more attention to form than is paid at present in conducting medico-legal inspections, and drawing up reports. A gentleman of the name of Connolly was indicted for the murder of a constable. While the constable, with several others, was attempting to apprehend him under a warrant, which, it seems, was drawn up and executed rather irregularly, Connolly fired among them through a door, and wounded one of them in the arm. The wound was so severe, that immediate amputation was deemed indispensable; but the unfortunate man expired, notwithstanding, not long afterwards. Connolly was tried upon two indictments, first for murder, upon the verdict of the Coroner's inquisition, and then for assault, on the bill found by the Grand Jury. On the first indictment his counsel objected, that the verdict of the Coroner's inquisition did not describe the wound with sufficient accuracy, because it merely called it a wound "about two inches and a half long." The Judge (Best, J.) sustained the objection, observing that the term *about* was too indefinite, and that, as such an inaccuracy would not be admitted in a special demurrer, he could not consent to receive it where doing so might decide the prisoner's death. The prosecutor was therefore obliged to desert the charge of murder. It does not appear to whom this inaccuracy of the Coroner's inquisition was owing; but we are well aware, that inaccuracies of far greater magnitude are frequently committed by medical reporters in similar criminal cases; and the foregoing history may serve as a useful lesson to them, as well as to coroners.—(*London Courier*.)

Tests for Arsenic. (Annals of Philosophy, Jan. and Feb. 1824.)—The following improvements in the mode of using the tests for this poison have been suggested by Mr R. Phillips of London, and Dr Traill of Liverpool. 1. For executing the process of reduction and sublimation, almost every work on chemistry and on medical jurisprudence advises the analyst to expose the mixture of the black flux and oxyde, for ten or fifteen minutes, to the heat of a charcoal choffer, in a coated tube, a quarter of an inch in diameter, and eight or ten inches long. Both Mr Phillips and Dr Traill very judiciously condemn this process as clumsy and unnecessarily complex. Dr Traill proposes to subject the mixture to the heat of a blowpipe lamp, in a small and naked tube, and says he has thus detected a tenth part of a grain. This mode of procedure we have been always in the practice of following, and have experienced no difficulty in procuring an obvious metallic crust from a sixteenth part of a grain. Mr Phillips, however, advises the substitution of the ordinary flame of a spirit-lamp, which answers admirably, for it does not disperse the sublimed metal so much as the blowpipe. As tubes are not always at hand, he recommends the use of a long ten-drachm phial, and describes minutely the mode of employing it. He ought to remember, however, that a tube is not always at hand only where the experimenter is not a chemist, and that the expedient recommended will succeed, like all other shifts, only in the hands of a practised person. With the ten-drachm phial, the experiment will often fail, if the quantity be minute; for the sublimate will be diffused too much to be distinctly recognised. 2. Mr Phillips has given a simple process for preparing a solution of sulphuretted hydrogen. Authors seem universally to prefer the solution of the gas, but for what reason we know not. We have certainly found a stream of the gas itself to be quite as delicate; and it is of course much more easily applied, as the solution cannot be kept for occasional use. 3. He has suggested a very neat improvement on the cuprous test. Add the solution of potass to that of the sulphate of copper; a blue precipitate is thrown down. If an arsenical solution be then added also, the precipitate becomes grass-green or apple-green. The change, we find, takes place slowly, unless the quantity of arsenic be large. 4. When the arsenic is dissolved in coloured fluids, it is difficult or impossible to obtain the true precipitates with the liquid tests. To remove the colour, Professor Orfila recommends the previous use of chlorine in solution. This, however, has the disadvantage of rendering the silver test difficult of application, because the chlorine throws down the silver in abundance; and, besides, we have observed, that, although the colour was destroyed in the solution, it often partly reappears in the precipitate, so as to give it a dingy hue. Mr Phillips proposes to destroy the colour with ivory-black. "On considering this part of the subject, it appeared to me that animal charcoal (ivory-black) might be advantageously employed for the purpose of destroying the colouring matter. I therefore mixed some of it with a coloured solution of arsenious acid, namely, the *Liquor Arsenicalis* of the London Pharmacopœia: I found that the colouring matter was so completely destroyed in a few minutes, that the test of nitrate of silver, or any other, might be readily applied. I repeated this experiment with port wine, gravy-soup, and a strong decoction of onions, and succeeded, in these cases, in procuring a solution sufficiently colourless for the application of the most delicate tests. It might be supposed, that the phosphoric acid, which the animal charcoal contains, might have some share in the production of the yellow precipitate with silver. I found, however, that water or wine, which was digested on the animal charcoal, produced no effect with the nitrate of silver, excepting a slight precipitate of chloride; and to prevent this, the ivory-black should be washed on a filter with boiling distilled water, till the fluid running through ceases to be affected by nitrate of silver. If, however, the ivory-black be of good quality, this precaution is unnecessary."

Extraction of Poison from the Stomach.—For some time past, the students and newsmen of London have been admiring an experiment performed by Sir Astley Cooper, on the extraction of poison by the syringe from the living stomach. The instrument employed was a peculiar syringe invented for the purpose, the subject a dog, the experiment amply successful. A similar process had been tried by others also, but without attracting much notice. As soon, however, as it passed through the hands of Sir Astley, it began to make a great noise: We have also been informed by Mr Calthorpe Williams, a pupil of this University, that, in December last, the remedy was employed with complete success by his brother, under the direction of Dr Payne of Nottingham, in the case of a woman who had poisoned herself with laudanum; and if our memory serves us correctly, we have observed, in the Journals, other instances in which it was successfully applied in the human subject. As it is held, therefore, in such repute, and indeed really appears to be a valuable remedy, and as our brethren seem ready to quarrel respecting the glory of discovering it, we extract the following lines from Dr Beck's Elements of Medical Jurisprudence. "Dr Physick, of Philadelphia, published a paper in 1812, in which he mentions that he *successfully* applied the syringe to a child poisoned with laudanum, and Dr Dorsey afterwards cured two individuals by the same treatment. That distinguished surgeon, however, subsequently states, that Dr Alexander Monro, secundus, first suggested the invention in 1797, although he (Dr Physick) was ignorant of this fact when he applied it practically. I conceive Dr Physick is entitled to the *honour of having been the first who saved life by its means.*" In an Inaugural Dissertation, printed at Paris, in 1801, by Cassimir Renault, on the Antidotes for Arsenic, we find also the following passage. "It is long since physicians proposed to introduce fluids artificially into the stomach: Boerhaave has expressed himself clearly on the subject. (Præl. Acad. vi. 588.) But I am not aware that any one has yet thought of emptying this organ mechanically, and without the aid of any vital power. Yet nothing was easier to imagine, for the very instruments used to fill it might with facility be modified so as to empty it. Let the elastic gum tube be made long enough to reach the bottom of the stomach, and wide enough to transmit soft half-digested matters, and adapt a metallic virol to its upper end, to be received into the pipe of a syringe. Then, having introduced the tube into the stomach by the mouth or nostrils, fit the syringe to it, and inject gently a certain quantity of liquid to dilute, suspend, or dissolve the poison. Next, by pulling the piston, draw into the syringe as much of the contents of the stomach as it will hold. When this double operation has been repeated several times, the stomach will be well washed, and the whole poison extracted without a shock, almost without pain, and in a very short space of time. Whenever the poison has not passed the pylorus, and is not in large fragments, the possibility of thus extracting it must be apparent to every person acquainted with physics. The process will probably be employed very extensively as soon as it shall have been tried with success on man. Meanwhile I have made the following experiment on animals. I have injected water, sometimes to the extent of eight ounces, into the stomachs of several little dogs, and I have always been able to pump it out entirely by the mode recommended. Indeed, the thing could not fail to fall out so, if we consider how successfully analagous means are used for emptying the bladder when it is filled with coagulated blood." (pp. 93–5.)

FOREIGN.

Influence of the Eighth Pair of Nerves on Digestion. (*Thèse soutenue à la Faculté de Médecine de Paris, par —— Vavasseur,* 1823.) M. Vavasseur, in the course of a set of experiments, by MM. Breschet, Mylne Edwards, and himself, on the influence of the nerves on the different functions, has had occasion to repeat the important researches of Messrs Brodie, W. Philip, and Broughton, concerning the effects of the division of the pneumo-gastric nerves on digestion; and the experience of the French physiologists has amply confirmed, and even extended the conclusions drawn by those of London. As the results at which they have arrived are of vast importance in respect to the physiology of the nervous system and of digestion, we shall make no apology for detailing them, although they have all been for some time before the public in the works of the British experimenters. 1. They first found, that when the conjoined pneumo-gastric and sympathetic nerves were simply divided in the neck of a dog seven hours after eating, the food was far less altered by digestion than in another animal fed in the same manner, and killed after the same interval, but not operated upon like the former. "The food, however, was somewhat pulpy towards the pylorus, and digestion had evidently begun, as the lacteals contained chyle." 2. They next observed, that when a portion, two inches and a half long, was removed from the conjoined nerves on each side of the neck in a dog newly fed, the alimentary matter, 10 hours afterwards, was hardly altered, had a clean, very dry appearance, and was merely besmeared on its surface with a thin coating of chyme. In another dog, treated precisely in the same manner, except that the nerves were simply divided, digestion had advanced very far. 3. Magendie had imagined, that the cause of digestion being impaired or prevented by the section of the pneumo-gastric nerves in the neck, might be the injury done to the respiration by the division of the pulmonary nerves; because he found that the food underwent the usual changes, if the section of the nerves was performed lower down than the giving off of the pulmonary plexus. M. Vavasseur and his coadjutors have shewn this supposition to be groundless; for the changes produced by digestion were completely prevented, by tying a double ligature at the bottom of the gullet and dividing it between. 4. Wilson Philip having observed, that the destruction of the lower part of the spinal cord impeded digestion, was led to infer that it is diminished by any cause which weakens the nervous energy. This conclusion has been satisfactorily confirmed by the Parisian experimenters, not only by the repetition of Philip's experiment, but likewise by the effects of injuries done to the brain, either through external force or by internal agents. When a portion of each hemisphere of the brain was removed in a dog newly fed, the food in eight hours was less altered than after the simple section of the pneumo-gastric nerves. And when an animal newly fed was plunged into a state of stupor by the injection of opium into the veins, the food, though considerably changed in eight hours, was much less altered than in another animal not brought under the influence of opium. "We see then," they conclude, "that whenever the intensity of the nervous influence is diminished, digestion is sensibly retarded. Whether the intensity be lessened by destroying a part of the brain or spine, or by bringing the system under the action of stupifying drugs, or by preventing the transmission of the nervous fluid along the nerves which conduct it to the stomach, the same results are uniformly obtained." 5. The last object of their attention was the alleged power of the galvanic fluid to effect the same changes as the nervous influence does on the food in the stomach. They first experimented on the horse. In one, immediately after it had fed on oats, the nerves were divided on both sides of the neck, and a portion removed two inches and a half long. In another this was done on one side only, while the opposite nerve was simply divided, and the lower end detached from its connexions, surrounded with a plate of lead, and connected with one side of a battery of 50 plates. The other side of the battery was then connected to another plate of lead, which was placed in the cavity of the abdomen upon the stomach; and the experimenters satisfied themselves, that the galvanic stream was kept up continuously by the frequent slight contractions of the *panniculus carnosus.* The three animals were killed seven hours after they had been fed. In that which was not operated on at all, the stomach contained a grayish mucilaginous fluid, without any grains; the small intestines a yellow mucosity; the cœcum a large quantity of altered and unaltered grains. In the animal which was galvanized, the stomach contained a small quantity of fluid and some altered grains; and the rest had passed entirely into the cœcum, where the grains were partly soft and chymous, and partly collected in balls. In that whose nerves were divided, but which was not galvanized, the stomach contained a large quantity of clear yellow liquid, and three times as many grains as in the second; and of these the greater part was unaltered, the remainder half digested; the cœcum contained merely a watery fluid and shreds of chaff, manifestly the residue of a former digestion. It is evident, that, in the last animal, none of the oats on which it had fed immediately before the operation had passed the pylorus; while the whole aliment in the unoperated animal, and at least two-thirds of it in that which was galvanized, had been transmitted into the intestines. The same experiment was repeated with similar results on the dog; and, consequently, we may now consider the statement first made by Wilson Philip, regarding the effect of galvanism in carrying on digestion, as firmly established. Of course, however, the establishment of this fact by no means establishes the inference he proposed to draw from it as to the identity of galvanic electricity with the nervous fluid.

Elementary Structure of the chief Animal Tissues.—(Thèse soutenue à la Faculté de Médecine de Paris, par H. Milne Edwards).—This thesis consists of a variety of interesting microscopical observations on the elementary structure of the tissues, by a young Englishman, brother of Dr Edwards, the well-known physiologist. The tissues which he has examined are the cellular, serous, mucous, muscular, tendinous, dermoid, nervous, and the coats of the vessels. The cellular tissue had been examined microscopically by Fontana alone, who describes it as consisting of tortuous cylinders, less than the finest red vessels. Dr Edwards finds, that, under a powerful microscope, the supposed cylinders are really lines of globules about 1-7600th part of an inch (1-300th of a millimètre), in diameter; that these lines are sometimes straight, sometimes bent, sometimes tortuous—are separated from each other by an obvious interval, which allows the subjacent layer to be seen betwixt them,—cross each others' course irregularly in every direction, and pass from one layer of the membrane to those above and beneath. This structure is precisely the same in man as in the dog, cock, frog, and carp; which were chosen as examples of the different classes of animals. In the peritoneum, in the mucous chorion, and in the conjunctiva, the size and arrangement of the globules is almost exactly similar. The author hints, that the free space, which exists between the lines of globules in each layer, accounts for the permeability of the cellular and serous membranes, lately maintained by M. Fodéra. The researches of Dr Edwards on the muscular tissue, confirm the statements made regarding it by former observers. A filament of the *biceps crucis* muscle was chosen for examination. It proved to consist of globules of the same size as in the cellular tissue, united in lines of various lengths, very nearly straight, and almost parallel with each other; and each line constituted an ultimate fibril, which could be isolated without changing the relative arrangement of its globules. The size of these globules does not vary with the age of the individual, as Muys alleges; it is the same in the fœtus as in the adult; and both in size and arrangement, they present the same appearance in man, in the sheep, the fowl, the frog, the carp, the lobster, and the beetle. The tendinous tissue, according to Fontana, is composed of fine threads, disposed in parallel, but undulated lines. Dr Edwards confirms this description, and adds, that the lines consist of globules of the same size as in the textures already mentioned. The only difference he could discover betwixt muscle and tendon is, that in the former the lines are straight, and in the latter undulated. The structure is identical in the round and flat tendons. The dermoid tissue consists of globules still of the same size, and arranged pretty nearly as in the tendinous tissue; the parallelism of the lines is not quite so exact. The observations on the coats of the arteries and veins are important. The external coat, being cellular tissue, has of course the same structure as that texture in other parts of the body; the inner coat is similar to the mucous tissue; and the middle coat precisely the same as the tendinous, and not composed of straight fibres, like the muscles; it is the same both in the arteries and in the veins, but in the veins it is very thin, and exists only in the large trunks. Several microscopical observers have investigated the elementary structure of the nervous tissue; but the most accurate previous researches are those of Mr Bauer, published not long ago by Sir E. Home. According to Mr Bauer, it consists of globules of three different sizes, arranged in parallel fibrils, exceedingly fragile, and united by a transparent elastic gelatinous-like fluid. Dr Edwards confirms the greater part of these facts; but he finds the globules to be all of one size, namely, the same as that observed by him in the globules of the other tissues. He also believes, that an adipose matter is interposed between the lines of globules; "for when a bit of nervous substance is crushed between two plates of glass, drops are disengaged, which are easily recognized to be fat." No difference exists between the cineritious and medullary matter, in the number, size, or arrangement of the globules; and the nervous structure is the same in the brain, in the nerves, and in the spinal cord of all the animals which were examined. The general conclusions, which the author deduces from his observations, are, that the size, form, and disposition of the elementary particles, are the same in all animals, and that their form and size are likewise the same in all the (soft) textures of the body.

On the accuracy of Dr Milne Edwards's microscopical researches, we shall not venture to offer an opinion; we presume no person is entitled to do so, who has not examined the same subjects with the same care, and with the same habits of observing, with which he appears to have laboured. We can safely say, however, that his memoir contains nothing to excite distrust; he does not begin it, (according to the general practice of the nation where he has fixed his residence), by undervaluing and setting aside the labours of all former physiologists; and it is written in a spirit of unostentatious good faith, which his adopted countrymen would do well to imitate, and which cannot fail to win the confidence of the reader.

Facts in favour of the contagious nature of Croup.—(*Arch. Gén. de Médecine, Décembre*, 1823. iii. 536).—We are not aware that any serious suspicion is entertained of this deadly disease being contagious or infectious. The two following facts, however, have led several practitioners in France to entertain an idea that it may propagate itself under certain unknown circumstances; and, though very far from being decisive, they are worthy of being recorded, that inquiry may be excited on the subject. The first set of facts is contained in a former number of the same Journal, (II. 625), and is stated on good authority. A woman having endeavoured to re-animate her child, newly dead of croup, by blowing into its mouth, was seized four days afterwards with the fibrinous form of laryngeal inflammation. The physician who attended her, and who relates the story, one morning, after examining her throat very narrowly, felt all day a gangrenous odour, and at night was also attacked with the same form of the disease.

The other set of facts furnishes better evidence. A child three years old was attacked with tracheal inflammation, running on to gangrene, and terminating fatally on the sixth day. A lady who was very assiduous in her attendance upon this child, took ill three days before its death, and died in the same manner, after five days' illness. A child who lived opposite the first patient, and who used often to visit him, was attacked on the day after his death, and likewise perished in twelve days. And, finally, the physician who attended the two last patients, was seized, in his turn, with a very violent cynanche, which yielded, however, to general and local depletion, without running on to gangrene, as in the three other cases. There has been evidently some peculiarity in these cases; for gangrene is a very rare termination of tracheal inflammation.

Proposal to substitute the Cyanuret of Potassium for the Hydrocyanic Acid in Medicine. (Journ. de Pharmacie, Avril 1823.)—The greatest inconvenience which attends the use of the hydrocyanic or prussic acid in medicine, is the uncertainty caused in its strength by its spontaneous decomposition. To remedy this inconvenience, Messrs Robiquet and Villermé have proposed to substitute for it its combination with potass, procured by decomposing the hydrocyanate of potass and iron with the aid of a strong and continued heat. When so treated, the iron separates, and a part of the cyano-gen is decomposed, while the remainder escapes decomposition, and remains united with the potassium. The cyanuret of potassium may then be separated from the iron and carbonaceous matter by the action of water, which dissolves it, and at the same time converts it into the hydrocyanate of potass. This solution the authors find to be not nearly so decomposable as the diluted hydrocyanic acid itself; and as for the dry cyanuret, it may be kept for an indefinite period, if secluded from moisture. They also find, that the cyanuret and hydrocyanate, formed in this way, possess all the formidable qualities of the hydrocyanic acid. The cyanuret impresses the tongue with a sense of freshness, soon succeeded by acridity: the dissolved hydrocyanate has a cool fresh taste, and like that of bitter almonds, succeeded by slight acridity, if concentrated. A solution of one grain of the cyanuret has as strong a taste as a drop of Gay-Lussac's acid similarly diluted: and a quarter of a grain, in the state of a concentrated solution, causes a momentary palsy of the tongue. A few grains of the cyanuret, placed on the tongue of a dog, produced signs of inflammation, and even traces of corrosion. A tenth part of a grain killed a linnet in sixty seconds, and less than one grain killed a guinea-pig in two or three minutes. One drop of the hydrocyanate, which contained the hundredth part of a grain, killed a linnet in thirty seconds; and half a dram, containing five grains, killed a very large old pointer in a quarter of an hour. These results as to its poisonous properties are conformable with those obtained some time ago by Westrumb of Hameln. (Journ. Complement. xvi. 255.) They leave little doubt that the hydrocyanate of potass will be likewise found to possess all the medicinal virtues enjoyed by the hydrocyanic acid.

On the Cause of the Yellowness of the Skin in Yellow Fever. (Journ. de Physiologie, Juillet 1823, *III.* 255.)—M. Desmoulins alleges, that the cause of the jaundiced appearance of the skin in yellow fever, as well as of the peculiar matter which is discharged by vomiting and purging in that disease, is not absorption or overflow of bile, but the exhalation of some of the principles of the blood modified in a peculiar manner. Several authors before him have advanced the supposition, that the matters of vomiting and the alvine discharges are the result of a peculiar secretion from the mucous membrane of the alimentary; and that idea is now very generally adopted. His other position, viz. that the yellow colour of the skin is nothing more than a sort of ecchymosis of the subcutaneous cellular tissue, he rests on dissections of that tissue in persons who had died of yellow fever—on comparative dissections in animals killed by a sort of yellow fever, which had been induced by injecting putrid fluids into the veins—on the absence of an augmented hepatic secretion in children born jaundiced,—and on the failure of M. Lassaigne to detect by analysis any of the principles of the bile in the serum of a child born in the same condition. We shall notice shortly the appearances he found in man and animals killed by yellow fever. In July 1815, a man, who arrived at Havre from Martinique after a passage of 22 days, took ill two days afterwards, and died in five days, under symptoms of yellow fever. These were violent headach, agitation, high delirium, alternate loss and recovery of vision, hiccup, hemorrhage from the nose, mouth and anus, yellow sweat, ecchymosis of the skin, sudden invasion of general jaundice on the third day, followed by coma, partial convulsions and carphology. The body was examined five or six hours after death. The skin appeared most deeply yellow on the cheeks, armpits, groins, and wherever the cellular tissue abounds; when cut, a little blood flowed from it as during life; when divided down to the subjacent cellular tissue, air rushed from the incision with a hissing noise; and then the tissue itself was found minutely injected with blood, like the inflamed conjunctiva of the eye, and of a reddish-brown colour. This was particularly well marked in the cheek, arm-pits, and groins. The stomach, heart, and colon, contained the same black, pitchy matter, which had been discharged by stool and vomiting; the whole intestinal canal had a brownish-red colour, interspersed with spots of a deeper tint; the liver shewed nothing peculiar; and the gall-bladder contained a moderate quantity of brownish-yellow bile. He then compares with the foregoing statement the appearances observed by M. Gaspard in dogs that had died of a similar disease, in consequence of the injection into their veins of water tainted by putrefying animal or vegetable matter. (See Journal de Physiologie, Jan. 1822.) M. Gaspard found petechial spots in the tissue of the heart, spleen, mesenteric glands, and subcutaneous cellular tissue, inflammation of the digestive canal, and effusion into it of a bloody gelatinous matter, like wine-lees; and in one experiment particularly, the skin and subcutaneous tissue were injected with blood as if they had been inflamed. The symptoms under which these animals had laboured were very similar to those of yellow fever, except that the skin was not distinctly yellow; and this difference (supposing Desmoulin's views of the cause of the colour to be correct) may be accounted for from the greater thickness of the integuments, which would prevent the subjacent ecchymosis from shining through.

On the employment of Strychnia and Brucia in Palsy. [*Journ. de Physiologie, Juillet* 1823. *III.* 266.] M. Andral proposes to substitute for the strychnia, or vegetable alkali of nux vomica, which has been recently introduced as a remedy for palsy, the brucia or alkaline principle of the spurious Angustura bark. The latter principle possesses precisely the same properties as the other, but it is at least twenty times less energetic; and consequently slight deviations from the proper dose would be attended with less severe consequences. He found it necessary, in using the strychnia, to begin with a twelfth part of a grain for a dose, and he could scarcely give with safety more than one grain, and that only in a patient who seemed peculiarly insensible to its action. In one case, the first pill of the least size caused a pretty severe attack of lock-jaw, and two produced strong contractions of the arms. It is therefore evident, that the effect of this dangerous drug varies with different constitutions, and consequently, if it is to be used at all, some mode must be discovered of giving it in such a manner as to cause the least risk from slight accidental errors in its dose. M. Andral's observations tend to prove that the brucia, besides possessing all the poisonous properties of the strychnia, is likewise endowed with its therapeutic qualities; and it is much safer, because much weaker. But the cases he has recorded by no means realise the flattering prospects which were formerly held out regarding the efficacy of either of these drugs in palsy. They show unequivocally, that neither can be used safely in cases of cerebral palsy, because, if any organic change has been produced in the brain, they have a strong tendency to excite inflammation of the surrounding parenchyma. It likewise appears, that in many cases of palsy, in which no such change has taken place in the cerebral structure, as for example in the palsy caused by lead, they completely fail in their effect. Yet, so far as his researches go, the practitioner has sufficient encouragement to make trial of the remedies, in that form of the disease. Of nine cases, three were cured, three more or less relieved, and three not at all improved. Of the three cases cured, none had been palsied longer than two months; the duration of the disease not having been reported in the others, it is possible that the author renders the comparative statement less favourable to the new remedy than the actual fact. For, in a very old and inveterate case, *attended with great emaciation of the muscles*, we presume none of the admirers of the strychnia would expect to procure with it any sensible amelioration, at least in so short a period as M. Andral allowed in his experiments. We should also caution those who may be induced to make trial of this mode of treatment, that, as far as we can judge from the few accurate cases that have been published (from those of Andral with the rest), its success is uniformly associated with the development of the peculiar constitutional action of the drug—that is, to give it a fair trial, it must be continued till it begins to cause trismus, or slight spasmodic twitches in the healthy muscles; for the restoration of the power of the palsied muscles seems never to take place till these effects are manifested. In short their occurrence is a sign that the system has then, and then only, been brought under the influence of the medicine. This is a consideration which we do not think has been taken into account by many who have experimented and written on the subject; and it is likewise one which will deservedly add to the caution of a practitioner in resolving to have recourse to such a mode of treatment.

Composition of the lately discovered Vegetable Principles. (*Annales de Chim. et de Phys. Oct.* 1823, xxiv. 191.) MM. Dumas and Pelletier have subjected the vegetable alkalis, and some others of the recently discovered principles, to ultimate analysis; and the following Table shews the results at which they have arrived. The process employed was that by the deutoxide of copper; and excessive precautions were taken in executing every step of it.

	Carbon.	*Hydrogen.*	*Oxygen.*	*Azote.*
Quina	75.00	6.66	10.43	8.45
Cinchonia	76.97	6.22	7.79	9.02
Brucia	75.04	6.52	11.21	7.22
Strychnia	78.22	6.54	6.38	8.92
Veratra	66.75	8.54	19.60	5.04
Emeta	64.57	7.77	22.95	4.30
Morphia	72.02	7.01	14.84	5.53
Narcotia	68.88	5.91	18.00	7.21
Caféin	46.51	4.81	27.14	21.54

It will be seen from the Table that all these substances contain a sensible quantity of azote. Notwithstanding the extreme care with which they seem to have proceeded, we confess that this result staggers our faith in their accuracy. Morphia, according to the analysis of Dr Thomas Thomson, contains no azote; and we may add, that in several very careful experiments performed at Paris by Dr Christison, under the direction of M. Robiquet, with the view of ascertaining whether azote existed in that alkali, the quantity of azote never amounted to more than a third of a part per cent. It must be evident to those acquainted with the steps followed in the process of analysis by the deutoxide of copper, that the experimenter may obtain more, but never less azote than existed in the substance decomposed.

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